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ART. LX. — *On the Transmission of Sensation and Volition through the Nerves.* Contribution from the Physical Laboratory of the Cornell University;* by M. M. GARVER, B.S., Professor of Natural Science in Mercersburg College, Pa.

DURING the winter of 1875–6 some experiments were made in the Physical Laboratory of the Cornell University, under the direction of Professor Wm. A. Anthony, to determine the rate of transmission through the nerves. The experiments were continued for a period of several months, during which some interesting and, as far as the writer's knowledge goes, new results were obtained.

The apparatus used for measuring the time was a modified form of the Schultz Chronograph and consisted essentially of a tuning-fork making 128·1 vibrations per second, a metallic cylinder covered with smoked paper, and an induction coil for giving and recording signals. The cylinder was capable of being rotated freely about a horizontal axis by means of a handle. The prolonged axis of the cylinder was cut with a screw and worked in a fixed nut; consequently when the handle was turned the cylinder gradually advanced in the direction of its axis. The tuning-fork, which was kept in vibration and regulated by one of König's automatic break-pieces, bore upon one of its prongs a flexible style of brass which was placed in contact with the blackened paper. Then when the tuning-fork was set to vibrating and the cylinder

*This article consists principally of extracts from a paper presented by the author for the degree of B.S. at the annual commencement of the Cornell University, June, 1876. For that reason the original name is retained, although the matter relating to "volition" has been almost entirely omitted.

turned, an undulating line was traced spirally round the cylinder; each one of the undulations corresponding to a known interval of time. The base of the tuning-fork was connected with one wire from the induction coil and the cylinder with the other, so that when the primary circuit was broken a spark would pass from the end of the style through the paper to the cylinder, displacing the lampblack in its passage and leaving a small dot in the undulating line or trace.

The method of using the apparatus was as follows: the wire of the secondary circuit connected with the tuning-fork was conducted to one side and used to give the signals by placing a short break in the wire in contact with the part of the body desired, the flesh of the body completing the circuit. The resistance in the primary circuit was so regulated that the signal could be distinctly felt without being painful. Then in the primary circuit two keys were necessary, one for the operator with which to give the signal and one for the individual experimented upon with which to answer the signal. The operator's key was so arranged that the circuit could be broken and closed again by simply depressing the lever. The answering key consisted of a light vertical lever, capped with a small insulating knob of ivory having a groove in its upper surface into which the nail of the index finger was placed in order that the circuit could be broken with the least possible lost motion, by simply flexing the finger. All the contacts of the keys were of platinum.

When everything was in readiness, the operator turned the cylinder and depressed his key; the spark passed, giving the signal and dotting the paper. As soon as the signal was perceived, the finger was flexed, another spark passed and was also registered upon the smoked paper. Then the time between receiving and answering the signal was found by counting the number of vibrations which were registered between the two dots,—the fractions of vibrations being estimated in tenths by the eye.

It was found that the time required to perceive and answer a signal even when given at the same point, varied considerably; consequently large numbers of observations were necessary in order to attain to anything like accuracy in the results.

The first series of observations were taken from the cheek and foot, and the difference in time as computed from the means was 0·0412 of a second. The distance was about five and a half feet, consequently the velocity was about 133·7 feet per second. Afterwards, however, more careful measurements were made, and the difference in time found between the foot and hip, hand and shoulder, neck and upper lumbar region, sight and hearing, and the probable errors of the results computed according to the usual method for such cases.

In experimenting upon myself the time required to answer a signal given on the left hand, as deduced from one hundred and nine single observations, was found to be $0{\cdot}1572'' \pm {\cdot}0009''$; when the signal was given upon the shoulder, the time as obtained from sixty observations, was $0{\cdot}1482'' \pm {\cdot}0010''$. The difference is $0{\cdot}009''$ and the distance, as measured, twenty-three inches, hence the velocity from the data, was in this case $213{\cdot} \pm 28{\cdot}6$ feet per second.

The results can probably best be shown in a table (see table I.) All the series from which these results were obtained are not given because they would swell the proportions of this article to an undue extent; and also because the establishment of the reliability of the numerical results obtained, it is expected, can be shown to be of minor importance.

TABLE I.

Person experimented upon.	No. of observations.	Point of application of signal.	Time required to answer.	Difference in time.	Length of nerve.	Velocity in feet per second.
Fowler (1),	30	Left foot,	$0{\cdot}1707''$	$\cdot0412''$	$5\frac{1}{2}$ ft.	$133{\cdot}7$
	46	Left cheek,	$0{\cdot}1296$			
Garver (2),	109	Left hand,	$0{\cdot}1572'' \pm {\cdot}0009''$	$\cdot009''$	23 in.	$213 \pm 28{\cdot}6$
	60	Left shoulder,	$0{\cdot}1482'' \pm {\cdot}0010''$			
Lee (3),	53	Left hand,	$0{\cdot}1686'' \pm {\cdot}0017''$	$\cdot01546''$	21 in.	$113{\cdot}2 \pm 17{\cdot}4$
	51	Left shoulder,	$0{\cdot}1532'' \pm {\cdot}0017''$			
(4),	103	Left foot,	$0{\cdot}1866'' \pm {\cdot}0014''$	$\cdot0111''$	36 in.	$270{\cdot}7 \pm 45$
	105	Left hip,	$0{\cdot}1755'' \pm {\cdot}0012''$			
(5),	95	Neck,	$0{\cdot}1508'' \pm {\cdot}0009''$	$\cdot0130''$	$16\frac{1}{2}$ in.	$105{\cdot}5 \pm 10{\cdot}1$
	90	Back,	$0{\cdot}1638'' \pm {\cdot}0008''$			
(6),	84	Left foot,	$0{\cdot}1927'' \pm {\cdot}0014''$	$\cdot0098''$	36 in.	$307{\cdot}4 \pm 47{\cdot}6$
	93	Left hip,	$0{\cdot}1829'' \pm {\cdot}0010''$			
(7),	210	Left hand,	$0{\cdot}15425''$?	$\cdot00003''$	23 in.	$60000{\cdot}$?
	196	Left shoulder,	$0{\cdot}15423''$?			

It will be noticed that the time in (4) and (6) differs considerably in the two sets of observations, although taken from the same person. The difference can hardly be owing to errors in observation, for the sum of the probable errors is less than $\cdot003''$, while the differences referred to are $\cdot0061''$ and $\cdot0074''$. There was evidently a change in the conducting power of the nerve, or in the mental status of the individual during the interval of time which elapsed before the last series were taken. Number (4) was taken Jan. 8th; number (6) a week later.

Number (7) gives an extraordinary result. It is so very large that it can hardly be regarded otherwise than as erroneous. Doubtless there was a change in condition while taking the observations, for all the observations from the hand were taken before any were taken from the shoulder, and the fatigue incident upon taking so large a number of observations is considerable, so that it can hardly be claimed that the experiments

were made under exactly the same conditions. If, however, the observations had been taken alternately from hand and shoulder, any change in condition would have affected both alike, and although the mean time might have increased or diminished, the differences would not have been materially affected. It is unfortunate that the order of the observations was not preserved, for then the whole series could have been broken up, and the change, if any, detected. This series will be again referred to hereafter.

Experiments were also made to determine the relative time required for receiving sensations through the eye and ear. Table II exhibits the results obtained from four different individuals.

TABLE II.

Name.	Time from "sight to hand."	Time from "ear to hand."	Difference in favor of the ear.
Lee	0·1628″ ± ·0011″	0·1327″ ± ·0009″	·0301″
	0·1793″ ± ·0012″	0·1359″ ± ·0019″	·0434″
Garver	0·1856″ ± ·0015″	0·1651″ ± ·0016″	·0205″
Brown	0·1808″ ± ·0035″	0·1364″ ± ·0020″	·0444″
Blake	0·1809″ ± ·0018″	0·1439″ ± ·0015″	·0370″

It will be noticed that the time required to hear a sound is in all cases *less* than that required to see a light; and the difference is sufficient to allow a sound to traverse a distance of from twenty to thirty feet and still be perceived as soon as a light emanating from the same point at the same instant.

In determining the time from "ear to hand," the induced spark from the coil was made to pass in close proximity to the ear, and the time registered as in the other cases. In finding the time from "sight to hand," it was necessary to eliminate the sound of the spark, and in order to accomplish this a small glass tube was fitted with platinum wires so as to give a spark about an inch in length, and partially exhausted of air. If the exhaustion be properly regulated, the spark is sharp and distinct, but perfectly noiseless.

It would seem that experimenters in attempting to determine the velocity of nervous transmission, have generally assumed that under the same circumstances, the rate of transmission is constant. Let us analyze a few series of observations and see what light they throw on the subject.

If the rate be constant, or if the determination of the rate involve no other elements than those found in measuring an ordinary fixed magnitude, the observations when plotted for the purpose of determining the accidental errors, should give a curve approximating to that shown in fig. 1, which is the well-known curve of probability. And inversely, it must be

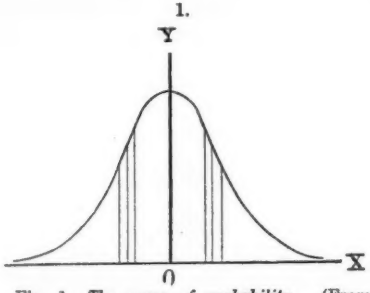


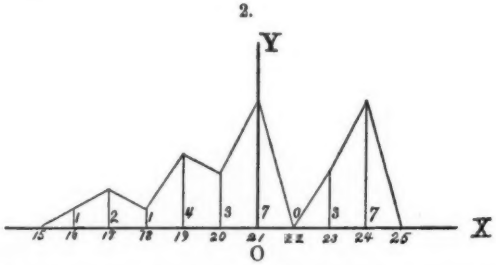
Fig. 1. *The curve of probability.* (From (Chauvenet.) Constructed from the equation $y=\frac{1}{\sqrt{\pi}}e^{-\Delta\Delta}$, where e is the base of the Napierian system of logarithms, y the *probability*, and Δ (i. e., x) the value of the errors. The scale of the ordinates is made four times that of the abscissæ in order to show the character of the curve more distinctly.

evident that if the "errors" are *not* accidental, but are governed by some other law, the form of the curve will vary accordingly, and may serve to give a clue to the new law.

The readings were taken in vibrations and tenths, the value of which was determined once for all. Let us take as examples two series where the signals were given on the left foot and answered as in every case by flexing the index finger of the right hand. The number of times each number appeared was as follows, the sign = being used instead of the word "appeared:"

Fowler. I.		Fowler, II.	
16=1	24=7	16= 2	22=1
17=2	25=0	17= 4	23=8
18=1	26=0	18= 4	24=2
19=4	27=0	19= 7	25=1
20=3	28=1	20=10	26=2
21=7	29=0	21= 7	
22=0	30=1		
23=3			

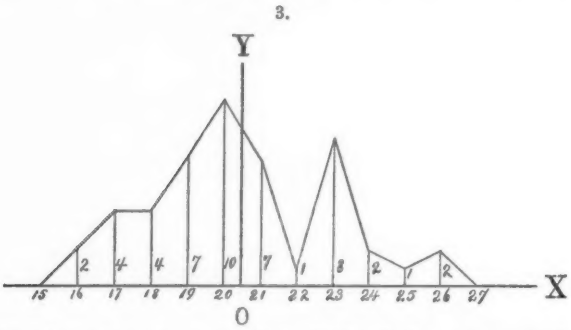
There were tenths in many instances, but for the sake of brevity they have been omitted.



Now if the curves representing these observations be plotted, they will be seen to have two or three maxima and minima, and evidently come under the second case. (See figs. 2 and 3).

The two principal maxima differ by three vibrations, i. e., by

about $\frac{1}{5}\frac{1}{0}$ of a second. And here is another peculiarity to which attention is particularly called, for it may serve to



explain the anomalous results obtained in some particular cases. The maxima in I (fig. 2) occur at 21 and 24, while in II (fig. 3) they appear at 20 and 23, showing an evident change in the condition of the nerve. The observations in II were taken the day after those in I, otherwise the conditions were apparently the same. It is impossible to tell when the change took place or to tell the cause, but if such a change should occur during the time occupied in taking the observations, it is manifest that the results would be materially affected and the peculiar *periodicity* of the results more or less destroyed.

A few more examples showing a periodicity will be given, but instead of plotting out in curves, it will be sufficient to indicate the groups by braces. The numbers at the right indicate approximately the difference between the means of the groups; $2\frac{1}{2}$ vibrations being equivalent to a little less than $\frac{1}{5}\frac{1}{0}$ of a second.*

III. Lee. "Sight to hand."	IV. Brown. "Sight to hand."		V. Garver. "Ear to hand."		VI. Blake. "Sight to hand."		VII. Lee. "Hand to hand."		VIII. Lee. "Shoulder to hand."	
19= 2	19= 1		17= 6		17= 1		16= 0		12= 1	
20= 4	**20= 4**		18= 2		18= 1		17= 2		. . . .	
21=10	21= 1	$2\frac{1}{2}$ vib.	19=11		19= 0		18= 1		16=2	
22=16	**22= 5**		**20=14**		20= 0		**19=10**		**17=9**	
23=12	**23= 4**		21= 9	3 vib.	21= 1		**20=11**	$2\frac{1}{2}$ vib.	18=9	$2\frac{1}{2}$ vib.
24= 4 ?	24= 1		22= 6		**22= 3**	2 vib.	21= 6		19=5	
25=10	25= 0		**23= 7**		23= 1		**22=10**		**20=9**	
26= 1	26= 1		24= 5		**24= 6**		23= 5		21=6	
27= 1	27= 2		25= 0		25= 1		24= 4		22=7	
28= 1	28= 1		26= 0		26= 1		25= 3		23=2	
			27= 2		27= 1		. . . .		24=1	
			28= 1		28= 0		30= 1		25=0	

* The exact rate of the fork during these experiments was 128·1 vibrations per second. Previous to the last experiments, in Dec., 1876, the fork underwent some repairs, after which its rate was 127·8.

The difference between VII and VIII is very marked, and by simple inspection, noticing the position of the maxima, we can determine pretty closely the difference in time between hand and shoulder. It appears to be two vibrations, corresponding very closely to the difference between the means, which was found to be 1·98 vibrations.

In the same way let us examine the experiments which gave the anomalous results before mentioned.

IX. Lee. "Hand to hand."

14= 0	23=10
15= 5	24= 6
16=10	25= 5
17=28	26= 4
18=40	27= 1
19=51	28= 1
20=22	. . . .
21=14	31= 1
22=12	

X. Lee. "Shoulder to hand."

14= 3	**19=32**	
15= 6	20=25	
16=11	21=21	5
17=28	22= 7	vib.
18=34	23= 7	
	24=11	
	25= 7	
	26= 1	$2\frac{1}{2}$
	27= 3	vib.

In X the series shows a tendency to break up into three groups, the means of the first two differing by something over five vibrations, corresponding to about $\frac{1}{2}\frac{1}{3}$ of a second; and this peculiarity will be sometimes noticed, that is, if the interval $\frac{1}{5}\frac{1}{0}$ of a second does not appear, an interval of *twice* that length may occur. In IX, however, no period is observable, and the series corresponds to the theoretical form of a series of observations to determine the value of a *fixed* magnitude. It has been shown how this might occur by the combination of series each periodic in its character; but whether this is so or not in this case, it is impossible to say. The means obtained from these two series are very nearly equal, and gave the doubtful results previously mentioned.

But it may be urged that the periods here shown are not very marked and may be caused by some imperfection in the apparatus employed. In that case I would refer to experiments made with entirely different apparatus, where the measurements depend upon other methods. A few examples out of the many which occur in Dr. Burckhardt's* work will be given. The readings are in millimeters and correspond to ·01 of a second; two millimeters then, being nearly equivalent to two and a half vibrations. The signal was a tactile one given by a light lever, and required in answer the movement of a stipulated muscle. The following, with the exception of the XIV, are taken from pp. 59 and 60.

The numbers in heavy type are those from which he estimated the "norm" or mean. The periodicity is evidently the

* Burckhardt: Die physiologische Diagnostik der Nervenkrankheiten, Leipzig, 1875.

same as that obtained in our experiments, and must have its foundation in the same cause.

XI. Right foot.	XII. Left foot.	XIII. Right hand.	XIV. Ear.	XV. XII dors. vert.
5=1	9=1	10=4	16=1	10=3
9=2	10=5	11=3	17=3	11=8
10=3	11=4	12=9	18=4	12=4
11=4	12=5	13=10	19=7	13=8
12=5	13=8	14=8	20=4	14=3
13=9	14=4	15=3	21=3	15=8
14=3	15=10	16=2	22=2	16=4
15=10	16=7	17=8	23=4	17=3
16=7	17=8	18=3	24=1	18=3
17=7	18=0	19=2		19=0
18=4	19=4			
19=3				

From the many examples of the occurrence of these periods, it would appear that they have their origin in the physical or mental action of the individual, but their physiological or psychological significance is not known, and the complete elucidation can be hoped for only through the aid of more extended investigations.

In December, 1876, another method was tried by which it was hoped that results having smaller probable errors could be obtained and at the same time throw light upon the periodicity of previous results. The experiments failed to fulfill expectations principally, perhaps, on account of the imperfect working of the apparatus, but nevertheless some important facts were learned. I am confident that the same or a similar method cannot fail to give interesting results if proper apparatus be employed.

In all the previous experiments the signals were given at *irregular* intervals so as to avoid anything like rhythm, thus requiring an act of perception and an act of volition for each observation; and from what has been shown, it appears that these two acts cannot be performed with any great degree of regularity. In order then, if possible, to obviate this difficulty, the signals were made to follow each other as nearly as may be with perfect isochronism. The method used was this:

With the same registering apparatus previously described, two induction coils were used, one of which in connection with the pendulum of a clock was made to give the signals at intervals of a second, each beat of the clock giving a signal which was registered upon the smoked surface. The other coil in connection with the answering key was used to register the answers. Two coils are necessary because when but one is used the answer is also felt and interferes with the rhythmical order of the signals. Part of the difference in time between the registered signals and answers must evidently be due to the delay caused by the passage of sensation through the nerve,

and, if the rate of transmission is uniform, will be proportional to the length of the nerve.

The first attempt was made by simply trying to beat time with the index finger of the right hand to signals given on the back of the left hand. Great care was taken to prevent any idea of the time being received *except through the nerves experimented upon.* On examining the register after the first set of observations was taken, it was found that the answers sometimes followed and sometimes preceded the signals and did not have the regularity expected. Upon noticing that the signal had been frequently anticipated, I made a new effort, taking great pains to be sure that the signal was *felt* each time before answering. A few observations were then taken, and after stopping a short time to examine the effect, a few more were taken. The following was the result, the tuning-fork making 127·8 vibrations per second.

XVI.		XVII.	
36	33	46	45·2
38·5	28·4	43	49
44	32·4	31·2	42·7
30	33·7	40	42·5
35	35·5	41	43·5
38·8	33·5	36·5	42
33·5		43	49
Mean=34·8		49	42 5
		36·2	47·4
		Mean=42·8	

Here then, we have two sets of observations taken within a few minutes of each other, the external conditions to all intents and purposes exactly alike, yet differing from each other in a marked degree; the mean of the second (XVII), exceeding the mean of the first (XVI) by nearly 25 per cent. The difference is probably due, at least in part, to an error in judgment or inability to recognize the exact instant at which the signal was given.

XVIII.	XIX.	
32	42	31·7
35	44·5	32·4
29·8	44·2	27·3
33·7	40·5	23 7
34	38	25
35·4	31·3	26
35·6		
37·5		

Two more sets (XVIII and XIX) taken two days afterwards, under as nearly as possible the same external conditions, exhibit another phase. Here it is seen that XIX, taken a few minutes after XVIII, commenced as on the previous day, with a marked increase over the preceding values, but gradually decreased nearly one-half in comparatively few observations.

Not obtaining very satisfactory results on account of the imperfect working of one of the coils, which failed to register, the investigation was broken off at this point. The imperfection mentioned could not affect the accuracy of the results obtained; the coil would simply cease working. It gave "accurate results or none at all."

There are too few observations in any one of the last series, to give any marked indication of the periodicity noticed in the results obtained by the first method; but nevertheless, series XVII, on analysis, shows a tendency to break into three groups, differing by six and a half vibrations. Thus:

XVII.
Garver "Hand to hand."

31=1		43=3	
----		44=0	
36=2		45=1	
----		46=1	6½ vib.
40=1	6½ vib.	47=1	
41=1		48=0	
42=4		49=3	

It is very improbable, to say the least, that certain values should be *selected* and certain others be rejected in this way without some cause beyond that of mere accident.

The period, it will be noticed, differs somewhat in different individuals but is almost constant in the same individual. The doubling may be caused by the obliteration of one of the normal groups, or as it appears sometimes, by the rejection of the intermediate values.

It may not be amiss to suggest an explanation of the "periodicity" however liable it may be to be overthrown by further investigation.

It seems that when an individual is experimented upon as in the given cases, he is conscious of being *surprised* by the signal even when expecting it. And sometimes the surprise is such that he forgets to answer until he is conscious of considerable time elapsing. At times he has to "think twice" before he moves his finger or stipulated muscle.

My own experience is that even when concentrating the utmost attention upon the point of application of the signal, I was sometimes aware that the signal was not answered as soon as it might have been; while at other times no appreciable time appeared to pass before answering. Knowing then, that there is a variable element entering after consciousness, it might not be going too far to assume that the variation is entirely cerebral.

Could not such a periodicity have its origin in the transformation lying between sensation and volition? It is readily conceivable that such might be the case and be of such a nature as to resemble an "increment to the judgment."

Mercersburg, March, 1878.

ART. LXI.—*The Upper Devonian Rocks of Southwest Pennsylvania;* by JOHN J. STEVENSON, Professor of Geology in the University of New York.

THE Vespertine or Pocono sandstone of the Pennsylvania Survey is a massive sandstone from 350 to 450 feet thick in southwestern Pennsylvania, and is the lower division of the Lower Carboniferous rocks. In Fayette and Westmoreland Counties, under Laurel and Chestnut Ridges, the last of the Alleghany Ridges toward the west, the Vespertine sandstone rests immediately upon a mass of gray to reddish-gray sandstones, interstratified with red to gray and olive shales.

These, representing the upper portion of the Devonian, are well exposed in the gaps of the Conemaugh River through the two ridges, as well as in the similar gaps made by the Youghiogheny River. In Laurel Ridge there are a few imperfect exposures in the deeper gorges made by the larger streams within Westmoreland and northern Fayette. In Chestnut Ridge there are no exposures of the Devonian in Westmoreland County, but in Fayette, south from the Youghiogheny River, these rocks are shown in the gap made by Dunbar Creek, as well as on the National Road, and in the deep gorges made by Shute's Run, Redstone Creek and the principal tributaries to Sandy Creek.

Lithologically, the transition from the Vespertine or Pocono sandstone to the Devonian rocks is sufficiently distinct. The great sandstone breaks down into a sandy shale, interstratified with some argillaceous shale, which in turn becomes merged into the well-defined red-gray to olive shales and sandstones representing the Upper Devonian. During the hasty examination of 1876, I was unable to make any close study of the section, and so provisionally regarded the lower rocks as belonging to the same series with the upper. This conclusion was given in my report to Professor Lesley for 1876. But the examinations made in the several gaps during 1877 showed the previous conclusion to be erroneous, and that the lower portion of the section from the very base of the Pocono sandstone is Devonian and not Lower Carboniferous.

A general section of the Devonian rocks, as observed in the gaps mentioned above, is as follows:—

1.	Shales and thin gray sandstones	80 feet.
2.	White to reddish-gray sandstones with some shale	70 "
3.	Reddish-gray micaceous sandstones with red to gray and olive shales	150 "
4.	Red to gray shaly sandstones with variegated clays and shales	200 "
	Total	500 "

Lithologically, the top portion, No. 1 of the section, is a transition mass, more closely related to the overlying than to the underlying rocks. But its relations are clearly shown by the fossils which occur in it. This part of the section is well exposed in the several gaps referred to, as well as on the National Road as it winds up the western side of Chestnut Ridge in Fayette County. At all localities examined it shows the same character, the sandstones are light-gray to brown and in thin beds, while the shales vary from brown to dull blue.

By far the greater part of the rest of the section consists of shale and shaly sandstone, in almost equal proportions and in alternating layers, from two to four feet thick. The sandstones are exceedingly micaceous, and, on the Conemaugh River, are for the most part little more than a compact micaceous mud; but in both gaps of the Youghiogheny there are compact gray sandstones, good enough to be used for building purposes. On the National Road, ten or twelve miles south from the Youghiogheny River, the shale and micaceous sandstones re-appear as on the Conemaugh. These micaceous sandstones are reddish-brown, have a concretionary structure and for the most part break down readily on exposure to the weather. Characteristic fossils are found throughout the section and many of the harder layers have their upper surfaces covered by a close mat of fucoids.

A curious conglomerate, from ten to twenty feet thick, occurs near the middle of the section and seems to be persistent, having been seen under Chestnut Ridge on the Conemaugh and Youghiogheny rivers as well as on the National Road. It is very much like that at the base of the Vespertine or Pocono sandstone, but the pebbles are not flattened and they are much larger than those seen in any other conglomerate exposed within southwest Pennsylvania. They are oval, thoroughly rounded and polished as by long rolling in water. Most of the larger pebbles are quartz, but with them are others of felsite-porphyry, quite soft, which had been blackened exteriorly before they were embedded in the material cementing the mass.

Relations of these Rocks.

In the final report of the First Geological Survey of Pennsylvania, Formation IX, the red Catskill of New York, is mentioned as occurring in the district under consideration. Following that report, I intimated in my second annual report to Professor Lesley that the rocks described in this article might be referred to that formation; on the maps accompanying my third annual report, now passing through the press, the areas are colored as Catskill. This, which was done to pre-

serve unity in the maps of the survey, is not in full accord with the facts.

To determine the relations of rocks one may be guided by lithological characters and relative position, or if possible he may trace the rocks to some typical locality, or should fossils be present he may make his determinations by means of those. For the most part, geologists are satisfied to abide by the last test, as it is of universal application and saves a great expenditure of time and labor. But some geologists are disposed to think the simpler method inaccurate, and seem inclined to rebel against an imagined assumption on the part of paleontologists. It is desirable then to ascertain whether or not the relations of these rocks can be determined by tracing or by lithological characters.

The bold anticlinal axes of southwest Pennsylvania are the Alleghany Mountains, Negro Mountain, the Viaduct axis, Laurel Ridge and Chestnut Ridge, all mountainous for the greater portion of their extent within the State of Pennsylvania. Under these axes alone may one look for exposures of the lower strata, for away from them the surface rocks belong to the Coal Measures.

An exposure under the Alleghanies in Maryland reaches below the Pocono or Vespertine sandstone, but northward there is no described exposure anywhere on the west side of those mountains in Somerset County of Pennsylvania; and, as far as can be ascertained from the report of Mr. Platt's close survey, the deepest gorge on that side is cut down only to the rocks of Formation XI, the Umbral. But in Cambria County, which is immediately north from Somerset, the exposures extend below Formation X, the Vespertine.

Negro Mountain separates itself from the Alleghany Mountains in northern Maryland and passes through Somerset County of Pennsylvania, dying out in southern Cambria, as may be learned from the reports of the Messrs. Platt upon those counties. No exposure in this ridge extends below Formation X.

The Viaduct axis separates itself from the Negro Mountain in northern Maryland and continues as a strong axis through Somerset, Cambria and Clearfield Counties of Pennsylvania. But it nowhere shows anything below the upper portion of Formation X, as appears from the reports made by Messrs. F. and W. G. Platt.

Laurel Ridge, at the line between Pennsylvania and West Virginia, exposes only the upper portion of the Umbral, XI, but at the Youghiogheny River, the upper part of the Devonian is reached, its section being exposed by the railroad cuts. Here and there, northward from the Youghiogheny, a deep

gorge is cut down to the Devonian, but owing to the thick coat of debris, no exposures occur and no section can be obtained south from the Conemaugh River. The fold declines north from that river, so that the gaps made within Cambria County by Chest and Black Lick Creeks reach barely to Formation X, and no gap in Clearfield County, south from that of the west branch of the Susquehanna, seems to expose any lower rock. These facts are gathered from the reports of Messrs. F. and W. G. Platt on Cambria, Somerset and Clearfield Counties, and from my own careful observations in Fayette and Westmoreland.

Chestnut Ridge first shows the Devonian rocks near the National Road in Fayette County, but thence northward the axis diminishes in strength, a given stratum being fully 1,000 feet lower at the Conemaugh than at the National Road. Between that road and the Youghiogheny River, several gorges are cut down to the Devonian, but no section can be obtained until the Youghiogheny River is reached. North from the river, owing to the decline of the axis in that direction, the deepest gorges soon fail to reach the Devonian and no exposure exists between the Youghiogheny and the Conemaugh. North from the Conemaugh the fold still decreases in strength, as is well shown by the fact that the Lower Coals creep constantly higher up its sides, so that the gaps made by Black Lick and other streams cannot do more than barely to reach Formation X, especially since the great Conglomerate of XII thickens very materially in that direction, as abundantly appears from the report on Clearfield County by Mr. Franklin Platt.

There is no exposure whatever for more than fifty miles along the west slope of the Alleghany Mountain; no exposure occurs in Negro Mountain or the Viaduct axis, so that no exhibition of Devonian Rocks appears between the Alleghanies and Laurel Ridge, a distance of twenty-five miles in an east and west direction; there are three exposures within seventy-five miles along Laurel Ridge, the intervals being forty and thirty-five miles; while in Chestnut Ridge there are three exposures within sixty miles, the intervals being ten and thirty-five miles. Surely under such circumstances one may hesitate before accepting any conclusion based on mere stratigraphy.

But is lithology any better? At all exposures to which reference has been made, except those in Clearfield County, respecting which I have no knowledge, rocks more or less similar in appearance are found immediately below Formation X, which is believed to represent the gray Catskill of New York. As they are at the top of the Devonian, they are likely to be

Catskill or Chemung, or to represent both groups, unless indeed those have thinned out. Professor H. D. Rogers thus describes the Chemung and Catskill of Pennsylvania:—

"VERGENT SERIES.

"VERGENT FLAGS (*Portage flags of New York*).—A rather fine-grained gray sandstone in thin layers, parted by thin alternating bands of shale. It abounds in marine *vegetation*. Thickness in Huntingdon 1,700 feet.

"VERGENT SHALES (*Chemung group of New York*).—A thick mass of gray, blue and olive-colored shales, and gray and brown sandstones. The sandstones predominate in the upper part, where the shales contain many fossils. Thickness in Huntingdon 3,200 feet.

"PONENT SERIES.

"PONENT RED SANDSTONE (*Catskill group of New York*).—In its fullest development this is a mass of very thick alternating red shales with red and gray argillaceous sandstones. It has very few organic remains. Among them is *Holoptychius*, and one or two other remarkable fossil fishes, of genera distinctive of Old Red Sandstone. This formation has its maximum thickness in its southeastern outcrops, where it measures more than 5,000 feet."—*Final Rep. First Geol. Surv. Penn.*, vol. 1, p. 108.

On pages 140, 141 and 142 of the same volume, Professor Rogers gives some further details respecting the lithological characters of the rocks. In the northwest belt, the Vergent or Portage flags consist of dark gray flaggy sandstones parted by thin layers of blue shale, with large marine plants and a *Nucula* as the chief fossils, while in the next belt toward the west they are made up of thin-bedded, fine-grained, siliceous gray sandstones, intimately alternating with blue and greenish shales.

In the middle belt, the Vergent Shales or the Chemung consists of gray, red to olive sandy shales, with gray and red argillaceous sandstones, but no details are given respecting this group in the belts west or northwest from the Alleghany Mountains.

In the northwest belt, the Ponent or Catskill consists of fine and argillaceous sandstones, with an increase of red and green shale and with some calcareous layers.

On page 793 of vol. ii of the same report, Professor Rogers points out the similarity between the deposits of the Ponent and Vergent, and states that the sediments of the former are quite as impalpable as are those of the latter.

If all these descriptions be compared with those already given of the rocks occurring in the gaps of the Youghiogheny

and Conemaugh through Laurel and Chestnut Ridges, it will be seen that, as far as lithological characters are concerned, those rocks may be either Catskill or Chemung, though indeed the evidence seems to be rather in favor of their being Chemung, for if one wished to describe them briefly and comprehensively, he could do no better than to combine Professor Rogers' descriptions of the Portage and Chemung, thus:—

"A rather fine-grained gray and brown sandstone in thin layers parted by alternating bands of gray, blue, olive and red shales. It abounds in marine vegetation, and in the upper part the shales contain many fossils."

Since it would be excessively difficult to determine the relations of these rocks by mere stratigraphy, and since the lithological characters fail to throw any distinct light upon the matter, the third test must be employed.

What are the fossils?

In the Summer of 1877, while making examinations in the Conemaugh Gap through Chestnut Ridge, I found, almost midway in the section given on another page, numerous specimens of *Spirifer Verneuilii*, *Rhynchonella Stephani* and *Streptorhynchus Chemungensis*, associated with many lamellibranchs and poorly preserved brachiopods, which could not be determined at the time. Further examination showed that these species occur up to within eighteen inches of the undoubted Pocono sandstone, or Formation X. The same species were found in abundance on the National Road as well as in the Youghiogheny Gaps; and, at all localities, the harder layers at from 100 to 150 feet below that sandstone are covered by a thick mat of fucoids, many of which have very thick stems and are several feet long.

In order that no doubt might remain respecting these species, I sent some specimens to Professor Hall, who has made out the following list;—

1. *Lingula*, sp.; 2. *Discina grandis* or *D. Alleghaniensis;* 3. *Streptorhynchus Chemungensis;* 4. *Rhynchonella Stephani;* 5. *Spirifera Verneuilii;* 6. *Palæoneilo maxima;* 7. *Sanguinolites rigida;* 8. *S. clavulus;* 9. *S. ventricosa?* 10. *Mytilarca Chemungensis;* 11. *Pteronites*, sp.; 12. *Pteronites*, sp.; 13. *Actinodesma recta;* 14. New form, undt.; 15. *Orthoceras crotalum?*.

These were collected at one locality and in haste, the only object being to obtain a few specimens of the more common forms. Of the list, Nos. 6 and 13 are found in New York only in the Hamilton rocks, while No. 15 is very closely allied to a Hamilton species and may be identical with it; but respecting the other forms there is no doubt—they are Chemung. All of these forms occur also in the layers interstratified with those containing the fucoids. They are not stray specimens, such as might have been washed from the older into the newer rocks,

for they are found in great abundance throughout the section and they are as well preserved as Chemung fossils usually are in New York. With these are immense quantities of fucoids, such as are characteristic of the Portage or lower Chemung in New York. But in the whole section there is not an *Anodonta*, not a fish-plate, not any fossil of any sort which can in any way be identified as belonging to the red Catskill of New York.

It is more than probable that the section represents only the lower portion of the Chemung and that not only the red Catskill, but also the upper portion of the Chemung is wanting in this part of Pennsylvania.*

What then has become of the great Catskill group? The upper or gray Catskill is represented, no doubt, by the Pocono or Vespertine sandstone, but the lower or red Catskill has disappeared. Nor is this disappearance at all strange. It is simply what might have been expected.

Professor H. D. Rogers, on pp. 141 and 142 of vol. i, of his Final Report, shows with what rapidity the Ponent or red Catskill thins out toward the northwest; that it is 5,000 to 6,000 feet thick in the southeast belt; 2,500 to 1,000 feet in the northwest belt; and 400 to 0 feet in the fifth belt; the diminution in each belt being distinct as one goes northwest or even west. No details are given respecting the variations of the group in a due west direction or towards the southwest, most probably because no possibility of tracing the group existed then any more than now. The presence of Ponent rocks is incidentally mentioned in notes upon the southern Alleghanies and the gaps through Chestnut and Laurel Ridges, but these observations were evidently regarded as too detached and too unimportant to be of value, since no reference is made to them in the general summary of the group given in vol. i of the Final Report.

All the evidence points in one direction. It is impossible by any stratigraphical work to make direct connection between the localities under consideration and those where the age of the rocks is settled beyond dispute; the lithological characters of

* In the Proceedings of American Philosophical Society, vol. xvii, p. 270, it is stated that at 300 feet below the *Pittsburgh Coal bed*, or midway in the Lower Barren Series, certain Chemung fossils have been found. I have been advised that, while pleading the cause of the Chemung group in the gaps through Laurel and Chestnut Ridges, I would do well to explain how Chemung fossils happen to be present midway in the Coal measures.

No explanation is necessary further than to say that the species were wrongly identified. I have examined the specimens and have recognized the following species:—

Lophophyllum proliferum M'C., *Athyris subtilita* H., *Spirifer planoconvexus* Shum., *Orthis carbonaria* Swal., *Chonetes granulifera* Owen, *Productus pertenuis* Meek?, *Hemipronites crassus* M. and H., *Lima retifera* Shum., *Astartella vera* H.

These are usually thought to be quite characteristic of the Coal measures.

the rocks in question are much like those of the Chemung, while the fossils, both animal and vegetable, are unquestionably of Chemung age. But one conclusion remains—the rocks are Chemung and, as already stated, probably represent only the lower Chemung; the great Catskill group has so far thinned out, that it is represented only by its upper or gray member, the Vespertine of Pennsylvania.

ART. LXII.—*Research on the Absolute Unit of Electrical Resistance;* by HENRY A. ROWLAND, Professor of Physics in the Johns Hopkins University, Baltimore, Md.

[Concluded from page 336.]

The Circle.—The circle whose constant we have called G″ and which was around the galvanometer whose constant was G, was a large wooden one containing a single coil of No. 22 wire.* To prevent warping, it was laid up out of small pieces of wood with the grain in the direction of the circumference, and was carefully turned with a minute groove near one edge in which the wire could just lie. It was about 5·cm. broad, 1·8 thick and 82·7 cm. diameter. As the room had no fire in it, the circle remained perfect throughout the experiment. The wire was straightened by stretching and measured before placing on the circle, which last was done with great care to prevent stretching; after the experiment it was measured and found exact to $\frac{1}{10}$ mm.

The circle was adjusted parallel and concentric with the coils of the galvanometer, but at a distance of 1·1 cm. to one side, in order to allow the glass tube with the suspending fiber to pass. The length of wire was 259·58 cm. which gives a mean radius of 41·31344 cm. These data give G″= ·151925. Preliminary results were also obtained by use of another circle.

Chronometer.—To obtain the time of vibration, a marine chronometer giving mean solar time was used. The rate was only half a second per day.

Wheatstone bridge.—To compare the resistance of the circuit with the arbitrary German silver standard, a bridge on Jenkin's plan, made by Elliott of London, was used. A Thomson galvanometer with a single battery cell gave the means of accurately adjusting the resistance, one division of the scale representing one part in fifty thousand.

* In another part of my paper I have criticised the use of wooden circles for coils, but it is unobjectionable in the case of a single wire, especially when the needle is suspended near its center.

Thermometers.—Accurate thermometers graduated to half degrees were used for finding the temperature of the standard.

The arbitrary standard.—This was made of about seventy feet of German silver wire, mounted in the same way as the British Association Standard. Immediately after use, two copies, one in German silver and the other in platinum-silver alloy, were made. It had a resistance of about 35 ohms. The temperature was taken as 17° C.

To obtain the accurate resistance of this standard in ohms, I had two standards of 10 ohms and one of 1, 100, and 1,000 ohms. The 1-ohm, and one of the 10-ohm standards, were made by Elliott of London, and the others by Messrs. Warden, Muirhead and Clark of the same place. But on careful comparison I found that Warden, Muirhead and Clark's 10-ohm standard was 1·00171 times that of Messrs. Elliott Bros. On stating these facts to the two firms I met no response from the first firm, but the second kindly undertook to make me a standard which should be true by the standards in charge of Professor Maxwell at Cambridge.* At present I give the result of the comparison with these standards, as well as some others, and also with a set of resistance coils by Messrs. Elliott Bros.

Commutators.—No commutators except those having mercury connections were used, and those in the circuit whose resistance was determined were so constructed as to offer no appreciable resistance. The commutator by which the main current was reversed, could be operated in a fraction of a second, so as to cause no delay in the reversal.

Connecting wires.—These were of No. 22 or No. 16 wire and were all carefully twisted together. The insulation was tested and found to be excellent.

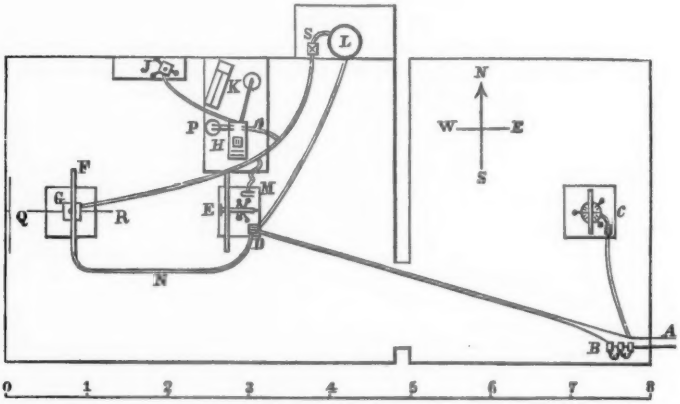
Inductor for damping.—This has already been described in my first paper on "Magnetic Permeability," and merely consisted of a small horse-shoe magnet with a sliding coil, which was introduced into the secondary circuit. By moving it back and forth, the induced current could be used to stop the vibrations of the needle and make it stationary at the zero point. This is necessary in the method where the first throw of the galvanometer needle constitutes the observation, but in the method of recoil it is not necessary to use it very often. I prefer the method of the first throw as a general rule, but I have used both methods.

This method of damping will be found much more efficient than that of the damping magnet as taught by Weber, and after practice a single movement will often bring the needle exactly to rest at the zero point.

* As this is nearly a year since, and as I cannot tell when the standard will arrive, I now publish the results as so far obtained, hoping to make a more exact comparison in future.

Arrangement of apparatus.—Two rooms on the ground floor of a small building near the University were set aside for the experiment, making a space 8 m. long by 3·7 m. wide. The plan of the arrangement is seen at fig. 1. The current from

1.



the battery, in the University, entered at A, the battery being eighteen one-gallon cells of a chromate battery, arranged two abreast and eight for tension. The resistance of the circuit was about 20 ohms, and of the whole battery about $\frac{1}{2}$ ohm, thus insuring a reasonably constant current.

At B some resistance could be inserted by withdrawing plugs so as to vary the current.

At C is the tangent galvanometer with commutator on a brick pier. The nearness of the commutator produces no error, seeing that we only wish to determine the ratio of two currents. The effect of currents in the commutator was, however, vanishingly small in any case.

At D is the principal commutator which reversed the current in the induction coils, L, or in the circle, F, when it was in the circuit.

The secondary circuit included the induction coil, L, the damping inductor, M, and the galvanometer G.

At H was the Jenkin's bridge, with standard at P, in a beaker of water, and a Thomson galvanometer at J K. The secondary circuit could be joined to the bridge by raising a U-shaped piece of wire out of the mercury cups.

The telescope and scale, E, were on a heavy wooden table, and the two galvanometers on brick piers with marble tops.

A row of gas-burners at Q illuminated the silvered scale in the most perfect manner.

Adjustments and tests.—The circle, F, must be parallel to coils of galvanometer, G. The circle and coils of galvanometer were first adjusted with their planes vertical and then adjusted in azimuth by measurement from the end of the bar, R, to the sides of the circle, F. The adjustment was always within 30′, which would only cause an error of one part in 25000.

The needle must hang in the magnetic meridian by a fiber without torsion, and the coils must be parallel to it. These adjustments were carefully made, but, as has been shown, the error from this source is compensated.

The needle must hang in the center of the galvanometer coils and on the axis of the circle. The error from this source is vanishingly small.

The scale must be perpendicular to the line joining the zero point and the galvanometer needle, it must be level and not too much below the galvanometer needle. All errors from this source are partially or entirely compensated by the method of experiment.

The induction coils, L, must be horizontal, and at the same level as the two galvanometers, so as not to produce any magnetic action on them. The error from this source is exactly compensated by this method of experiment, but could never amount to more than 1 part in 2000.

The tangent galvanometer should have the plane of its coils in the magnetic meridian, but all errors are compensated.

The connecting wires must be so twisted together and arranged as to produce no magnetic action, but tests were made in all cases where the error was not compensated, and found to be practically zero. The insulation of all coils, wires and commutators was carefully tested.

Method of Experiment.—As has been stated before, the method generally used was that of the first throw of the needle, though the method of recoil was also used. For the successful use of the first method a quickly vibrating needle and the damping inductor are indispensable, seeing that with a slow moving needle we can never be certain of its being at rest. By this method it is not necessary to have the needle at rest at the zero point, but, if it vibrates in an arc of only a millimeter or two, we have only to wait till it comes to rest at its point of greatest elongation on either side of the zero point and then reverse the commutator. The error by this method is in the direction of making the throw greater in proportion of the cosine of the phase to unity. The smallest throw used was 100 mm. Hence, if the needle vibrated through a total arc of 2 mm., the error would be 1 in 17,000. In reality the needle was always brought to rest much more nearly than this.

The method of recoil was used once with the needle vibrating

in 7·8 seconds, but the time of vibration was too short and another needle was constructed vibrating in 11·5 seconds, which was a sufficiently long period to be used successfully after practice.

There seems to be no error introduced by the time taken to reverse the commutator in the method of recoil, seeing that the breaking of the current stops the needle and the making starts it in the opposite direction. As the time was only a fraction of a second the error is minute in any case.

While the current is broken in the reversal, the battery may recuperate a little and there is also some action from the extra current, but there seems to be no doubt that long before the four or six seconds which the needle takes to reach its greatest elongation everything has again settled to its normal condition and the current resumes its original strength. Hence the error from these sources may be considered as vanishingly small.

Some experiments were made by simply breaking the current and they gave the same result as by reversal.

The following is the order of observations corresponding to each experiment.

1st. The time of vibration of needle was observed.

2d. The current was passed around the circle, F, so as to observe β and α. Simultaneous readings were taken at the two galvanometers. The commutator at the tangent galvanometer was then reversed and readings again taken. After that the commutator to the circle was reversed and the operation repeated. This gave four readings for the circle and eight for the tangent galvanometer, as both ends of the needle were read. In some cases these were increased to six and twelve respectively. This operation was repeated three times with currents of different strengths, constituting three observations each of α and β. To eliminate any action due to the induction coils, they were sometimes connected in one way and sometimes in the opposite way.

3d. The resistance of the circuit was adjusted equal to the arbitrary standard.

4th. The circle, F, was thrown out the circuit and the observations of θ and δ begun. Two throws, δ, one on either side of zero were observed and one reading of θ taken. The commutators at s and C were then reversed, and the operation repeated. This whole operation was then repeated with currents of three different strengths. The position of the two induction coils was now reversed and observations again made with the three currents. The resistance was now compared with the standard, the difference noted, and the resistance again adjusted. The observations were completed by turning the induction coils into the two other positions which they could occupy with

respect to each other, followed by another comparison of resistance with standard.

5th. Observations of α and β were again made as before.

6th. The time of vibration was again determined.

The observations as here explained furnished data for three computations of the resistance of the circuit, one with each of the three currents. In each of these three computations, α was the mean of 16 readings, β of 8 or sometimes 12, θ of 16 and δ of 16. In using the method of recoil nearly the same order was observed.

The time of vibration was determined by allowing the needle to vibrate for about ten seconds and making ten observations of transits before and after that period. During the experiment, I usually observed at the telescope and Mr. Jacques at the tangent galvanometer.

The methods of obtaining the corrections require no explanation.

Results.

The constant corrections are as follows for the first needle.

$$a = -\tfrac{1}{2}\lambda + \tfrac{5}{12}\lambda^2 = -\cdot00711$$
$$b = -\tfrac{1}{2}t = -\cdot00020$$
$$c = -\cdot000006$$
$$d = +\cdot000074 \text{ at } 20\cdot^\circ \text{ C.}$$
$$e = \frac{1}{128_n\lambda}\left(c_1^2 - c_n^2\right) = +\cdot00003$$
$$f = +\cdot00003$$
$$a+b+c+d+e+f = -\cdot00718.$$

For method of recoil it becomes $-\cdot00016$.

Hence for A and B, log K=11·4536030

" " A and C, log K=11·2852033

" " B and C, log K=11·1886619

For method of recoil using A and B, log K=11·4566630.

For second needle and method of recoil,

$$a = -\tfrac{1}{2}\left(\frac{\lambda}{\pi}\right)^2 = -\cdot000050$$
$$b = -\tfrac{1}{2}t = -\cdot00025$$
$$c = -\cdot000006$$
$$d = +\cdot000074$$
$$e = +\cdot00003$$
$$f = +\cdot00003$$
$$a+b+c+d+e+f = -\cdot00017$$

For A and B, log K=11·4566587

" A and C, log K=11·2882590

" B and C, log K=11·1917176

The distance of the mirror from the scale varied between 192·3 and 193·5 cm.

Table of Results.

Induction coils.	Method.	T	Tem. of Standard.	β	δ	α	θ	Corrections. A +·000	Corrections. B −·00	Corrections. C +·00	Corrections. D −·000	Resistance in earth quad. sec.	Mean resistance in earth quad. sec.	Error.	Percentage error.
A & C	0	7·800	18°·7	220·17	170·29	40° 12′·44	42° 32′·56	19	075	036	22	34·672			
	0	"	"	185·61	141·28	35 29·06	37 22·94	"	"	029	"	34·784	34·748	+·029	+·083
	0	"	"	173·83	131·40	33 43·56	35 28·01	"	"	025	"	34·787			
B & C	0	7·800	20·3	222·20	137·07	40 27·81	42 49·81	21	145	036	74	34·760			
	0	"	"	186·66	113·70	35 41·94	37 33·69	"	"	029	"	34·680	34·714	−·005	−·014
	0	"	"	174·64	105·71	33 55·87	35 35·94	"	"	025	"	34·702			
A & C	0	7·806	21·0	219 66	168·16	40 7·56	42 18·06	22	176	035	44	34·762			
	0	"	"	184·92	139·65	35 26·37	37 7·87	"	"	025	"	34·725	34·773	+·054	+·155
	0	"	"	173·20	129·84	33 40·75	35 13·06	"	"	024	"	34·831			
A & B	0	7·822	24·7	208·65	235·98	38 41·87	40 56·13	29	334	034	44	34·700			
	R	"	"	176·31	119·20	34 10·50	40 57·17	"	"	102	"	34·733	34·716	−·003	−·008
	R	"	"	165·20	117·48	32 30·56		"	"	126	"				
B & C	0	7·826	25·4	211·15	129·29	39 3·87	41 19·19	32	370	034	44	34·730			
	0	"	"	178·01	107·48	34 27·31	36 13·25	"	"	027	"	34·723	34·718	−·001	−·003
	0	"	"	166·96	100·27	32 46·12	34 19·87	"	"	025	"	34·700			
A & B	0	7·828	25·9	210·53	237·74	39 1·69	41 11·62	32	392	034	43	34·665			
	0	"	"	177·71	197·72	34 24·00	36 7·50	"	"	025	"	34·704	34·685	−·034	−·098
	0	"	"	166·32	184·35	32 40·00	34 14·56	"	"	024	"	34·686			
A & B	R	11·488	19·8	219·62	85·407	40 0·16	42 14·65	21	123	039	33	34·701			
	R	"	"	185·15	84·651	35 20·82		"	"	108	"	34·667	34·688	−·031	−·089
	R	"	"	173·64		33 37·31		"	"	129	"	34·697			
A & C	R	11·487	20·6	216·80	56·630	39 37·43	41 35·22	22	158	030	22	34·724			
	R	"	"	183·38	56·059	35 2·81		"	"	100	"	34·725	34·712	−·007	−·020
	R	"	"	172·92		33 31·25		"	"	121	"	34·688			

Mean, 34·7192 ± ·0070

Should we reject the quantity 34·831 in the third experiment so as to make the mean result of that experiment 34·744 instead of 34·773, we should obtain as a mean result of the whole

$$34{\cdot}7156 \pm {\cdot}0053,$$

which has a less probable error than when the above observation is retained. The number of plus and minus errors are also more nearly equal and the greatest difference from the mean 1 part in 1100. However the two results do not differ more than 1 part in 10,000.

We shall take

$$R = 34{\cdot}719 \pm {\cdot}007 \ \frac{\text{earth quad.}}{\text{second.}} \text{ at } 17^{\circ}\,\text{C.}$$

as the final result.

Discussion.

On glancing over the table we see that the number of negative errors greatly exceed the number of positive, but, if we take only the four errors which are greater than 1 part in 5,000, we shall find two of them negative and two positive.

Combining the results with the different coils we have

A and B	34·696 ± ·005
A and C	34·744 ± ·011
B and C	34·716 ± ·007

Had we no other results to go by, we might suppose that the value of M might not have been found as exactly for these coils as we have supposed them to be. But if we include the preliminary results rejected on account of the imperfect circle used, we shall find

A and B	34·704 ± ·006
A and C	34·718 ± ·017
B and C	34·758 ± ·016

which has the greatest error in an entirely different place.

From the first series the probable error of each determination of M is 1 in about 2,000. But as this includes the experimental errors which are about equal to $\pm\frac{1}{1\,0\,0\,0}$, the real probable error of M must be about 1 part in 2,500. The number of observations is however too small for an exact estimate of the probable errors.

Taking the results with currents of different strengths, we find

For strongest current	34·716
" medium "	34·715
" weakest "	34·727

which are almost perfectly accordant. Taking the results from the method of recoil and the ordinary method, we find

For ordinary method	34·726 ± ·010
" method of recoil	34·705 ± ·006

If the probable error is subtracted from the first and added to the second they will very nearly equal each other. Hence the difference is probably accidental. Indeed, by the combination of the results it does not seem possible to find any constant source of error, and therefore the errors should be eliminated by the combination of the results.

In the final result

$$R=34{\cdot}7192\pm{\cdot}0070$$

the probable error, $\pm{\cdot}0070$, includes all errors except the ratio of G to G″. We may estimate the probable error of G at $\pm\frac{1}{3\,0\,0\,0}$ and of G″ at $\pm\frac{1}{3\,0\,0\,0}$.

Hence the final probable error of R, including all variables, is $\pm\frac{1}{3\,3\,0\,0}$, or $\pm{\cdot}04$ per cent,

$$\text{or } R=34{\cdot}719\pm{\cdot}015.$$

The probable error of the British Association determination was $\pm{\cdot}08$ per cent, not including the probable error of the constants; and of Kohlrausch's determination $\pm{\cdot}33$ per cent, including constant errors.

Comparison with the Ohm.

The difficulty in obtaining proper standards for comparison has been explained above and I shall have to wait until the arrival of the new standard before making the exact comparison. At present I give the following results, which seem to warrant the rejection of Messrs. Elliott Bros'. 10-ohm standard and to make that of Messrs. Warden, Muirhead and Clark correct. I shall designate the coils by the letter of the firm and by the number of ohms. Experiment gave the following results:

W (10)=1·00171 × E (10), experiment of June 8, 1877.
W (10)=1·00166 × E (10), " " Feb. 23, 1878.

W (1,000) : W (100) :: W (10) : ·999876 E (1), experiment of February 23, 1878.

Now the greatest source of error in making coils is in passing from the unit to the higher numbers. As the reproduction of single units is a very simple process the single ohm is without much doubt correct, and as the above proportion is correct within one part in 8,000 of what it should be, it seems to point to the great exactness of the standards then used, seeing that the exactness of the proportion could hardly have been accidental. It is also to be noted that Messrs. Warden, Muirhead & Clark's 10-ohm standard agreed more exactly with a set of coils by Messrs. Elliott Bros. than their own unit E (10).

The resistance of my coil as derived from the different standards is as follows:

From Elliott Bros. resistance coils	.....	34·979 ohms.
" " " 10-ohm standard	.....	35·083 "
" W., M. & C.'s " "	.....	35·024 "
" W., M. & C.'s 100-ohm "	.....	35·035 "

These give for my determination the values of the ohm as follows:

From Elliott Bros. resistance coils,	·99257	$\frac{\text{earth quad.}}{\text{sec.}}$
" " " 10-ohm standard,	·98963	"
" W., M. & C.'s " "	·99129	"
" W., M. & C.'s 100-ohm "	·99098	"

For the reasons given above I accept the mean of the last two results as the value of the ohm.

To preserve my standard I have made two extra copies of it, the one in German silver and the other in platinum silver alloy. The comparisons are given below. No. 1 is in German silver and the other in platinum silver alloy. The temperature is 17·° C.

No. I	1·00034	June, 1877.
No. I	1·00029	Feb., 1878.
No. II	·99630	June, 1877.
No. II	·99932	Feb., 1878.

These are the values of the copies in terms of the original standard whose resistance is 34·719 $\frac{\text{earth quad.}}{\text{sec.}}$

From these results it would seem that the German silver of which the standard and No. I were composed was perfectly constant in resistance. The wire has been in my possession for several years and seems to have reached its constant state.

The final result of the experiment is

$$1 \text{ ohm} = ·9911 \frac{\text{earth quad.}}{\text{sec.}}$$

ART. LXIII.—*The Ancient Outlet of Great Salt Lake;* by A. C. PEALE.

IN this Journal for April, 1878, pp. 256–259, is an article entitled "The Ancient Outlet of Great Salt Lake: a letter to the editors by G. K. Gilbert." In this article Mr. Gilbert states that "previous to 1876 the outlet was not discovered, or if discovered, its position was not announced," and that "in the summer of that year" he "had the great pleasure of finding it in Idaho, at the north end of Cache Valley, the locality being known as Red Rock Pass." He says also that the announcement was made by him "without reservation in a communication to the Philosophical Society of Washington,"*

* At the 116th meeting of the Society, January 13, 1877, "Mr. G. K. Gilbert made a communication on Lake Bonneville, the great fossil lake of Utah. He described an ancient outlet of the lake at Red Rock Pass near the town of

and that the announcement was also made for him "in the same unequivocal manner" "in the Smithsonian Report for 1876" (p. 61), and "in Baird's Annual of Scientific Discovery for 1876"* (p. 206), and that "there seemed to be no occasion for further publication until the matter should receive its full discussion in the Reports of the Survey of which Professor Powell has charge," but owing to a statement † in this Journal for January, 1878, p. 65, "it seems proper" to him "to defend" his "*positive* assertions by setting forth the facts which appear" to him "to place the existence and position of the ancient outlet *beyond question.*"‡

As Red Rock Pass, the point of Mr. Gilbert's discovery (?), is within the area assigned during the season of 1877 to Mr. Gannett's division of the United States Geological Survey of the Territories, with which I was connected as geologist, it seems proper that I should call attention to several errors in Mr. Gilbert's statements.

In the first place, *his so-called discovery is not a discovery on his part.* The fact that Red Rock Pass was an outlet for the lake that once filled the Salt Lake Basin and adjoining valleys §

Oxford, Idaho, by which its waters were discharged into Snake River. During and since the desiccation of the lake, the land which it covered has been tilted to the northward in common with the region of the Laurentian lakes and the eastern and western seaboard." (Bulletin of the Philosophical Society of Washington for 1877, p. 103.)

* The "unequivocal" announcement referred to by Mr. Gilbert, is stated in exactly the same words in both publications, and is made in the following rather *vague* manner. "Before commencing the main work of the season, Mr. Gilbert made an excursion in search of the outlet of Lake Bonneville, the great fossil lake of Utah." * * * "The search for the point of outlet was successful, and it was found at the *north end of Cache Valley, a few miles beyond the boundary of Utah, in the Territory of Idaho.* (Annual Report of the Board of Regents of the Smithsonian Institution for 1876. Washington, 1877, p. 61) and (Annual Record of Science and Industry for 1876, edited by Spencer F. Baird. New York, 1877, p. 260)

† The statement referred to is the following: "It is believed that the explorations of the survey under the direction of Dr. Hayden, the past season, have determined the probable ancient outlet of the great lake that once filled the Salt Lake Basin." (This Journal, vol. xv, Jan., 1878, p. 65.)

‡ The italics in this paragraph are my own.

§ The following extracts from the Report of the Survey for 1870, written by Dr. Hayden, show that as early as that time the extent of the great inland basin and its real conditions were truly appreciated by him.

"Let us for a moment take a bird's-eye view of the great inland basin of which Salt Lake Valley forms only a part. We shall find that what is termed the Great Basin of the West comprises the vast area inclosed by the Wasatch Mountains on the east, and the Sierra Nevada on the west, the crest or water divide of the Columbia on the north, and that of the Colorado on the south. We shall also observe that this great region has no visible outlet; that it is composed of a multitude of smaller basins or valleys, each of which has its little lakes, springs and water-courses, their surplus water either evaporating or sinking beneath the surface. If we examine the elevations in this region, we observe a wonderful uniformity in the surface of the valleys, and find that none of them are much above the level of the waters of Great Salt Lake." (p. 172.)

"I infer that a vast fresh-water lake once occupied all this immense basin; that the smaller ranges of mountains were scattered over it as isolated islands,

was not only recognized, but well known five years prior to Mr. Gilbert's supposed discovery. On page 202 of the Annual Report of the Survey for 1872 * is the following statement by Professor F. H. Bradley: "The level of the divide between the head of Marsh Creek and the Bear River drainage, at Red Rock Pass, as ascertained by the party of 1871, indicates that this was probably another point of outflow;" and on the following page (203) the following sentence in relation to the terraces in Marsh Creek Valley, which extend northward from Red Rock Pass: "They are on too large a scale, and the valley is too wide, to have resulted from merely the drainage of the small area of mountains about the head of the stream; and I am strongly of the opinion that this must have been at one time the channel for a large outflow from the Great Basin."

It seems to me that this places the discovery where it belongs beyond question.

It appears also that Mr. Gilbert ignores some of his own statements. In his report to Lieutenant Wheeler,† he says that Professor O. C. Marsh informed him that he had discovered on the northern shore of the lake an outlet leading to Snake River, and in a foot note on the same page says, "Professor Frank H. Bradley mentions four points of possible outflow from the northeast margins.—(*United States Geological Survey of the Territories*, 1872, p. 202.)"

In the second place, *Red Rock Pass was not the outlet of Lake Bonneville.*

Lake Bonneville extended over the whole of Marsh Creek Valley and its outlet was more than forty miles farther north than Mr. Gilbert ever went. Red Rock Pass was only a point of stricture in the lake.

In his article (p. 257), Mr. Gilbert says: "In Marsh Creek Valley the eye seeks in vain for the familiar shore lines of the Salt Lake Basin, and the conclusion is irresistible that here the ancient lake outflowed." This sentence implies that he went into Marsh Creek Valley. Had he done so or had he even ascended one of the numerous points that command the view

their summits projecting above the surface; that the waters have gradually and slowly passed away by evaporation, and the terraces are left to reveal certain oscillations of level and the steps of progress toward the present order of things; and that the briny waters have concentrated in those lake basins, which have no outlet." (p. 170.)

This was written before Mr. Gilbert began his western investigations.

Dr. Hayden also first indicated Cache Valley as one of the bays of the ancient lake. (Report United States Geological Survey for 1871, p. 19.)

* Sixth Annual Report of the United States Geological Survey of the Territories for 1872, Washington, 1873.

† Report upon Geographical and Geological Explorations west of the 100th meridian, in charge of First Lieutenant Geo. M. Wheeler. Vol. iii, Geology. Washington, 1875, p. 91.

of both valleys (Cache, and Marsh Creek) the relation of the two must have been apparent.

The "gentle alluvial slopes" mentioned by Mr. Gilbert (on page 257) as being "divided for several miles by a steep-sided, flat-bottomed, trench-like passage a thousand feet broad, and descending northward from the divide" are *white sandstones* similar to those in the bottoms of Cache and other valleys of the Salt Lake Basin. The following elevations on the terraces in Marsh Creek Valley were obtained by barometrical observations.*

Two miles north of Red Rock Pass on the east side of the valley, 5,137 feet.

Six miles west of Red Rock Pass on the edge of Marsh Valley, 5,053 feet.

Twenty-six miles from Red Rock Pass on the west side of the valley, 5,117 feet.

The elevation of the Bonneville beach is 5,185·7 feet† and it is evident that the Red Rock Gap (the walls of which do not exceed the elevation of 5,000 feet) could not have been a barrier to Lake Bonneville. The conclusion is therefore irresistible that the result of Mr. Gilbert's four or five years' search is a mistake.

In the third place *Red Rock Pass was an outlet, but it was the outlet probably when the lake was at the level indicated by the Provo Beach.* When the barrier at the northern end of the Bonneville Lake was removed, that portion of the lake occupying Marsh Creek Valley was completely drained, and Red Rock Gap became the barrier of the lake that remained. Then it was that the course of Marsh Creek began to be outlined, and the lowering of the lake was doubtless comparatively rapid until the level of the Provo Lake was reached. The line of the Provo Beach indicates a period of comparative permanence, but when the pass became lower than the lake, of course the lake was drained. The elevation of the pass as obtained by railroad level is 4,792 feet, and the Provo Beach, according to Mr. Gilbert, is about 365 feet below the Bonneville Beach,‡ which would give an elevation of 4,820·7 feet for the former.

In the fourth place I wish to call attention to two more of Mr. Gilbert's statements. On page 258 he says, "In Dr. Hayden's Preliminary Report of the field work of his survey for

* I am indebted to Mr. Henry Gannett for all the elevations I use and for other valuable information obtained by him while in the field.

† This elevation is obtained by adding 967·7 feet (the height of the Bonneville Beach above Salt Lake, given by Mr. Gilbert in the Report of Geographical and Geological Explorations west of the 100th meridian, vol. iii, p. 92) to 4,218 feet, the elevation of Salt Lake, which was obtained from the chief engineer of the Union Pacific Railroad.

‡ This Journal, vol. xv, April, 1878, p. 258.

the season of 1877, noticed on page 56 of the current volume of this Journal, there is no mention of the observations at Red Rock Pass, but *the omission appears to have been accidental*, &c." The portion of this statement that I have italicised is a gratuitous assumption. The omission was *not* accidental. I did not believe that the outlet was at Red Rock Gap, and in the Preliminary Report (page 7), I made the following statement: "The lower valley of the Portneuf is interesting from the fact that it is the probable ancient outlet of the great lake that once filled the Salt Lake Basin."*

Mr. Gilbert also hopes that I "will not advocate in" my "report the idea that the divide between the Malade and Marsh Creek was one of the old outlets of the ancient Salt Lake when its waters were at the highest level."

Had I been writing a final report on the subject I would perhaps have used the word overflow instead of outlet. It seems, however, scarcely necessary to refer to this point, as Mr. Gilbert himself acknowledges that he "did not ascend to the summit, although" he "had undertaken last summer to examine every divide between the Columbia and Salt Lake Basins, that might have afforded passage to the water."†

In all his investigations he seems never to have noted any evidences of a lake having a higher level than his Lake Bonneville. Such evidences, however, do exist. On both sides of the Portneuf where it comes into Marsh Creek Valley an upper terrace is seen, and in 1872 Professor F. H. Bradley also readily identified an upper terrace in Marsh Creek Valley at the level of about 1,000 feet above the stream.‡ In Gentile Valley and in Cache Valley also, traces of this upper terrace exist. The elevation of the Malade divide is 5,650 feet and the level of this higher lake as indicated by the elevation of the terraces must have been between 5,500 and 5,800 feet, so that when the waters were at the highest level there was no impossibility of communication over the Malade divide.

I have read and re-read Professor Bradley's Report and I see no warrant for the statement that he made the "astonishing

* Preliminary Report of the field work of the United States Geological and Geographical Survey of the Territories for the season of 1877. Washington, 1877. p. 7.

† This Journal, vol. xv, April. 1878, p. 258. Mr. Gilbert appears to take it for granted that the point of outlet must necessarily be found at one of the existing divides between the Great Basin and the Columbia.

The railroad profile from the bluffs on Bear River in Cache Valley to Red Rock Pass (a distance of fifteen miles) shows a difference of elevation of only eleven feet, and the descent of Marsh Creek from Red Rock Pass to a point twenty-six miles farther north averages only 1·07 feet per mile. This indicates that the divide is not well marked and that previous to the erosion that followed the draining of Lake Bonneville it may easily have been much farther north. At present the divide for several miles is a swamp.

‡ Sixth Annual Report of the United States Geological Survey, for 1872. Washington, 1873, p. 203.

suggestion that four outflowing streams might have *coexisted.*" (The italics are my own.)

In conclusion I wish to state that this paper is based on the combined observations of Mr. Henry Gannett and myself, made during the progress of our regular field work without any special reference to finding an outlet of the ancient Salt Lake. Mr. Gilbert has spent portions of at least two seasons in the study of this special subject in the northern portion of the basin, and it is evident that his investigations are still unsatisfactory.

ART. LXIV.—*Note on the Ferment-theory of Nitrification;* by F. H. STORER, Harvard University.

THE results of the following experiments bear so immediately upon the recent observations of Schlösing* and Warington,† noticed in the April number of this Journal, that I am led to publish them by themselves, out of their legitimate connection with other experiments upon which I have been for some time engaged. My experiments were made for the purpose of testing the action of certain oxidizing agents on ammonium compounds, and they are, perhaps, not the less interesting as a contribution to the ferment-theory of nitrification, inasmuch as they were undertaken without any reference to the observations of Schlösing, and not at all with the view of supporting or even of testing his experiment.

For my first trial, eleven common glass bottles of greenish tint, each of 500 cc. capacity, and fitted with corks carrying inlet and outlet tubes, were charged as follows: No. 1 with distilled water that had been carefully freed‡ from nitrates, nitrites, and ammonium compounds; No. 2 with a solution of ammonium chloride, containing $\frac{2}{10}$ milligram of NH_3 to the centimeter; No. 3 same as No. 2, with the addition of a quantity of ferric hydrate; No. 4 same as No. 2, with the addition of ferrous hydrate; No. 5 same as No. 3, with the addition of charcoal made from white sugar; No. 6 ammonium chloride, black oxide of manganese and sugar charcoal; No. 7 leached peat and pure water; No. 8 leached peat and ammonium chloride; No. 9 leached peat and ferric hydrate; No. 10 leached peat, ferric hydrate, and ammonium chloride; No. 11 pure water.

Pure water was used in each case to dissolve or suspend the

* Comptes Rendus, lxxxiv, 301.

† Journal of London Chem Soc., 1878, i, 44.

‡ In the manner described in the note on page 182 of vol. xii of this Journal.

chemicals, and the absence of nitrates and nitrites was proved by testing each of the solutions and mixtures with iodo-starch, at the beginning of the experiment.

The bottles were about half filled with liquid, i. e., each of them contained about 250 cc. of the solution or mixture allotted to it.

The "leached peat" was prepared from some bog-meadow mud from the Bussey farm, which had been kept in barrels in a dry store-room for three or four years. This thoroughly air-dried substance was percolated with pure water until the filtrate gave no reaction for nitrites or nitrates.

The ferric and ferrous hydrates were used in the recently precipitated condition; they were made from the corresponding chlorides by precipitating with ammonia-water, in the cold.

The bottles were connected with one another, in the order indicated, with short pieces of caoutchouc tubing in such manner that by aspirating at No. 1 air could be made to bubble through the water in each member of the series. The corks of the bottles and the caoutchouc connectors were covered with shellac in alcohol so that the entire apparatus was completely air-tight. That is to say, none of the solutions had the least connection with the external air, excepting as it was purposely admitted at the inlet-tube. To purify the air that was drawn into the apparatus, it was made to pass through a long tube loosely filled with clean cotton wool; through two sets of Liebig bulb-tubes filled with a tolerably strong solution of yellow prussiate of potash, to remove ozone; through two sets of similar bulbs charged with potash-lye to remove nitrates and nitrites; through a dry bottle, to catch liquid drawn forward from the potash bulbs; and through a large drying tube charged for two-thirds its length with calcium chloride and one-third with soda-lime.

By means of a Bunsen filter-pump a rapid current of air from out-of doors was drawn incessantly night and day, through the series of bottles, for a fortnight, in September, 1877, in the direction from No. 11 to No. 1; at the end of which time the contents of each bottle were tested for nitrites and nitrates, by boiling with cadmium, distilling with acetic acid, and mixing iodo-zinc starch and acid with the distillate. 100 cc. of liquid were taken for each of the tests, and fifteen minutes were allowed in which to watch for the appearance of the blue coloration. Nothing could be more distinct and decisive than the results of these tests. The solutions from bottles Nos. 8, 9 and 10 namely gave immediate and strong reactions for the nitrogen oxides, while the contents of Nos. 1, 2, 3, 4, 5, 6, and 11 gave no reaction whatsoever. No. 7 gave a reaction, but not a very strong one. The reaction seemed to be strongest in

the contents of No. 10. It appeared from these results that there had been formation either of a nitrate or nitrite in each of the bottles which contained humus, and no such formation in either of the other bottles; but the thought suggested itself, that the small amount of nitrogen oxides found in No. 7 may perhaps have been dragged over mechanically from No. 8 by the current of air.

A second series of tests was made upon Nos. 8, 9 and 10, to see which of them gave the weakest reaction, 25 cc. of liquid being taken from each bottle and diluted with pure water to the volume of 100 cc. before applying the test. It appeared again that No. 10 gave the strongest reaction and that No. 8 gave the weakest. Roughly estimated, the strength of the reactions from jars Nos. 10, 9 and 8 were to one another as 5 : 2 : 1.

It may here be said that humus was employed in these experiments for the sake of testing the old observation of Millon,* who noticed that ammonium salts are changed to nitrates when in contact with oxidizing humus, and who argued that the chemical action originated by the coming together of humus and oxygen, was communicated to the ammonium compound. In his own words: "The oxidation of the humic acid is the cause of the oxidation of the ammonia."

In so far as the foregoing experiments go, we have manifestly a striking confirmation of the accuracy of Millon's work. But on repeating the experiments with the difference that the bog-earth was henceforth exhausted with hot, strong muriatic acid, before proceeding to wash it with water, no such results as the foregoing were obtained. For example, in a second series of trials, arranged like the first, the contents of bottles Nos. 1 to 6 were identically the same as before, only that the amount of liquid was now 150 cc. instead of 250 cc. No. 7 contained the purified peat mixed with water; No. 8 was the same as 7 plus a quantity of gypsum; No. 9 was the same as 8 plus some ammonium chloride; No. 10 contained water alone; No. 11 purified peat plus ammonium chloride and ferric hydrate; No. 12 well-washed cotton rags plus ammonium chloride and ferric hydrate; No. 13 purified peat plus ammonium chloride; No. 14 pure water; No. 15 pure water, the same namely, which had already done service (as No. 11), in the first series of experiments.

The weather (October 13, 1877), being too cold to permit the keeping open of a window, the current of air, purified as before, was drawn from the cold-air-box of the hot-air furnace used for warming the laboratory. The current of air passed this time in the direction from No. 1 to No. 15; it was maintained constantly

* Kopp and Will's Jahresbericht der Chemie, 1860, xiii, 101 and 1864, xvii, 158.

during ten days, and the contents of the bottles were then tested as before for nitrites and nitrates. But no reaction was obtained in either instance, with the exception of No. 12 (cotton rags, etc.), which gave a faint coloration of a not very satisfactory character. After the application of the test those of the bottles which still contained a sufficiency of liquid were re-attached to the aspirator and air was drawn through them continually during another week, when the test for nitrites and nitrates was again applied. But in no case was there any reaction, with the exception of bottle No. 8 whose contents gave a faint coloration.

Inasmuch as warm, and, at times, hot summer weather had prevailed during the term of the first series of experiments, while the out-door air was decidedly cool and that of the laboratory by no means very warm during the time allotted to the second series, a third set of trials was undertaken in December, 1877, in which the contents of the bottles were heated artificially. That is to say, the bottles were immersed in a large water-bath, which was heated to from 70° to 80° C. during the progress of the experiment.

In this third series, there were eight bottles containing the purified peat, first by itself; then admixed with gypsum; with gypsum and ammonium chloride; and with ammonium chloride and ferric hydrate. One bottle contained washed cotton rags, ammonium chloride, and ferric hydrate as before. The water-bath was heated during working hours for a fortnight, perhaps a hundred hours in all, and during this time air was drawn from the furnace-box and purified as before. The contents of the bottles were then tested for nitrites and nitrates, but no reaction was obtained in either case. To make sure that the absence of the reaction was not due to any interference caused by the presence of the peat or the chemicals, a fresh portion of liquid was taken from each of the bottles, enough nitrate of potash to amount to 0·001 gram of N_2O_5 was added, and the test for nitrites and nitrates was applied in the usual way: reactions were now obtained immediately in every instance.

In the light of the facts observed by Schlösing, the natural inference from the results of these experiments is that the formation of nitrates or nitrites in bottles Nos. 7 to 10 of the first series of experiments was due to the presence of living organisms which the peat had harbored, and that the absence of nitrification in the other series of experiments is to be attributed to the destruction of the ferment-germs by the hot acid with which the peat employed in these experiments had been treated. It is to be observed, moreover, that the formation of nitrogen oxides in the bottles Nos. 7 to 10 is in nowise out of

accord with the important fact observed by Warington, that darkness* is essential to the action of the nitrifying germs, for although my bottles were not shielded from diffused daylight, the mixtures of peat and water which they contained were practically dark-colored muds, not ill-fitted to shelter the germs from the light.

It is possible of course that the colder weather which prevailed during the later trials may have had an influence upon their results; but if this be so, the fact must be counted as an additional argument in favor of the ferment-theory. Moreover, it may fairly be inferred that if mere oxidation be all that is needed to induce nitrification, the heating of the liquids by day in the third series of experiments would have been sufficient; though both the strong heat and the cooling of the bottles by night would have tended to prevent the growth of living organisms.

In the interest of the ferment-theory, it would have been well to control the negative results above given by trials with mixtures of the purified peat and carbonate of lime, for peat which has been treated with muriatic acid has always a slight acid reaction, due I suppose to free humic acid, no matter how thoroughly it may have been washed with water, and it is to be supposed that this acid peat, devoid withal of phosphatic and other saline matters, is not favorable for the growth of the ferment. But as was said before, my experiments were made to test the oxidizing action of certain chemicals, not to cultivate living organisms.

It may be added that I have not as yet found any evidence

* The following statement has a certain interest for analysts as bearing upon the stability of dilute solutions of ammonium chloride. In February, 1877, it was noticed by my assistant, Mr. Lewis, that a solution of ammonium chloride which had been prepared ten or twelve months previously for use in connection with Nessler's test, by dissolving the salt at the rate of 3·15 grams to the liter, now gave a strong reaction for nitrites, although freshly-prepared solutions, made in the same way from like materials gave no reaction. The old solution was contained in a glass-stoppered bottle which was about half filled by it, and it had been kept most of the time in a dark cupboard.

Acting on the supposition that the change of the ammonium salt to a nitrite had been caused by the growth of some fungus in the liquid and that such fungus might perhaps be present in other bottles in the neighborhood, I collected as many different specimens of moulds as could be found growing in the various saline solutions kept in the laboratory, and, after rinsing with pure water, placed them in a series of half-gallon bottles, into which had been poured from half to quarter of a liter of dilute ammonium chloride, such as is used for Nesslerising. A quantity of mother of vinegar also was placed in a similar bottle. This series of bottles was left to stand during early autumn, and winter in a green-house, in a rather strong light, and the solutions were tested from time to time, at intervals of three or four weeks, but no reaction for nitrites or nitrates could ever be obtained from either of the bottles. The experiments of Warington show that these trials were vitiated by the strong light to which the bottles were exposed. It is not altogether unlikely that different results would have been reached if the bottles had been kept in the dark.

that solutions of ammonium compounds can be oxidized to nitrites or nitrates by means either of ferric oxide, of black oxide of manganese or of gypsum. I can say with Millon,* that, in spite of all that has been written in favor of the oxidation of ammonium by ferric oxide, "I owe it to truth to state that though the most varied attempts have been made to oxidize ammonia in the cold (i. e. in the wet way), by peroxide of iron, they have all proved unavailing."

I am indebted to my assistant, Mr. D. S. Lewis, for his careful attention to the details of these experiments.

Bussey Institution, Jamaica Plain, Mass., April, 1878.

ART. LXV.—*Geographical and Geological Survey of the Rocky Mountain Region under the direction of Professor J. W. Powell. Account of work performed during the year* 1877.

[Concluded from page 358.]

Ethnographic Work.—During the season the ethnographic work was more thoroughly organized and the aid of a large number of volunteer assistants living throughout the country was secured. Mr. W. H. Dall, of the United States Coast Survey, prepared a paper on the tribes of Alaska, and edited other papers on certain tribes of Oregon and Washington Territory. He also superintended the construction of an ethnographic map to accompany his paper, including on it the latest geographic determination from all available sources. His long residence and extended scientific labors in that region peculiarly fitted him for the task, and he has made a valuable contribution both to ethnology and geography. With the same volume was published a paper on the habits and customs of certain tribes of the State of Oregon and Washington Territory, prepared by the late Mr. George Gibbs, while he was engaged on scientific work in that region for the government. The volume also contains a Niskwalli vocabulary with extended grammatic notes, the last great work of the lamented author. In addition to the map above mentioned and prepared by Mr. Dall, a second was made, embracing the western portion of Washington Territory and the northern part of Oregon. The map includes the latest geographic information, and is colored to show the distribution of Indian tribes, chiefly from notes and maps left by Mr. Gibbs. Much of the linguistic material of this volume was collected by correspondents of the Smithsonian, and turned over to Professor Powell, to be consolidated with materials collected by members of his corps.

* Chemical News, 1860, ii, 337, from Comptes Rendus.

These papers form a quarto volume of 361 pages, entitled Contributions to North American Ethnology, volume I, the first of a series to be published on this subject.

Volume II, relating to the tribes of the eastern portion of Washington Territory and the State of Oregon, was partially prepared for the printer, but it was thought best to withhold its publication until further materials were collected from that region.

The third volume of the series has been published. This relates to the Indians of California. Mr. Stephen Powers, of Ohio, has been engaged for several years in the preparation of this volume. The first part contains an account of the habits, customs, mythology, etc., of the several tribes. At our earliest knowledge of the Indians of California they were divided into small chieftaincies, speaking diverse languages, and belonging to radically different stocks, and the whole subject was one of great complexity and interest. The materials collected by Mr. Powers were sufficient to successfully unravel the difficult problem relating to the classification and affinities of a very large number of tribes, and his account of their habits and customs is of much interest. A number of vocabularies of the Smithsonian Collection are published with those of Mr. Powers. The linguistic portion of the volume was edited by Professor Powell.

The volume is accompanied by a map of the State of California, compiled from the latest official sources, and colored to show the distribution of linguistic stocks.

The Rev. J. Owen Dorsey, of Maryland, has been engaged for more than a year in the preparation of a grammar and dictionary of the Ponka language. His residence among these Indians as a missionary has furnished him favorable opportunity for the necessary studies, and he has pushed forward the work with zeal and ability.

Professor Otis T. Mason, of Columbia College, has for the past year rendered the office much assistance in the study of the history and statistics of Indian tribes.

Brevet-Lieutenant Colonel Garrick Mallory, United States Army, has during the year been engaged in the study of the history and statistics of the Indians of the United States. His researches lead him to the conclusion that the generally received opinion that a very large Indian population occupied this country at the time of its discovery is erroneous, that the supposed rapid and general decadence of the Indians arising from contact with civilization is not sustained, and that when circumstances have not rendered it impossible, they are making reasonable progress towards civilization, together with which in many instances their numbers have increased. No final publi-

cation on the subject has yet been issued, but he has read papers before the Philosophical Society of Washington and other scientific bodies, to invite the attention of ethnologists to the subject. He has also been engaged in preparing the history and bibliography of the Klamath, Chinook, Wayiletpu, Sahaptin, and other families of Oregon, and his papers on this subject will appear in the second volume of Contributions to North American Ethnology.

In March last, Mr. Albert S. Gatschet was employed to assist in the study of Indian languages, and during the spring months his time was occupied as an assistant in compiling the bibliography of the North American languages. During the summer and autumn months he visited a number of tribes in Oregon, for the purpose of collecting vocabularies and grammatic notes. On his way to the field he stopped at Ogden, where he found a tribe of Shoshone Indians, from whom he procured a vocabulary of about five hundred words.

In Chico, Butte County, California, he stopped one week, to visit the Michópdo Indians, a branch of the Maidu stock, where he collected linguistic material of value. From Chico he proceeded directly to the Klamath Agency, in Southern Oregon, where eight weeks were devoted to the study of the language of the Klamath Indians, a branch of the Modok family. Mr. Gatschet had previously studied this language, by obtaining words from Modok Indians visiting Washington and New York, and his work at the Klamath Agency was a continuation of such study. Altogether he has collected a vocabulary of about five thousand words, also many sentences and texts on historic and mythologic subjects arranged with interlinear translations.

The numerical system of this language is quinary, and the numerals above eleven have incorporated particles giving them a gender or classifying significance, apparently based upon form. The subject and object pronouns are not incorporated in the verb; the personal pronouns differ from the possessive; and a true relative pronoun exists. An important characteristic of the language is the use of prefix-particles in nouns and verbs indicating form, and the reduplication of the first syllable, which is usually the radical syllable, for the purpose of showing distribution. It is often equivalent to our plural. It occurs in the singular of adjectives indicating shape and color, in augmentative and diminutive nouns and verbs, in iterative and frequentative verbs; and forms the distributive plural of many substantives, adjectives, numerals, verbs and adverbs.

From the Klamath Agency, Mr. Gatschet proceeded to the Grande Ronde Agency, in the northwestern part of Oregon. On his way he stopped at Dayton, and made collections of

Shasta and Umpqua words, from reliable Indians. On the Grande Ronde Agency are found a large number of tribes and remnants of tribes which were collected there after the Oregon war of 1855–6; and with the exception of the Klikatats they are all from Western Oregon. The following is a classification of the linguistic stocks now on this reservation: Tinnéh, Silets, Wayiletpu, Shasta, T'sinuk, Sahaptin, Selish, Modok and Kalapuya. The Kalapuya once occupied almost the whole extent of the beautiful and fertile Willamette valley, and one branch of this stock, the Yonkálla, even extended into the Umpqua valley.

The Tuálati language, a dialect of the Kalapuya stock, was the one studied by Mr. Gatschet, and from his notes the following characteristics appear: The phonetics are strikingly soft and harmonious, and though consonants are often assembled in large clusters, they never offend the ear, nor do they seem unpronounceable to Americans. A large number of words begin with vowels, especially with *a*, *i* and *u*. The substantive adjective and numerals are not inflected for case, as in the Modok. Adjectives and numerals and some substantives are varied to indicate the plural number. The parts of speech are very imperfectly differentiated. The personal and possessive pronouns have the same gender, as a distinction of sex is indicated in the singular of subject pronouns but not in the plural. Prefix particles are extensively used to express the mood, voice and tense of the verb, and the same particles fulfill this function in the noun. The personal pronouns of the direct object differ greatly from the pronouns of the indirect object; and every one of the three persons, in the singular and plural, possess a different series of direct and indirect objective pronouns. The conjugation of the transitive verb differs in many particulars from that of the intransitive. There appear to be structural affinities between the Kalapuya and Selish stocks. Over three thousand words, many hundred sentences and valuable ethnologic texts were collected.

Besides the Michópdo, Modok and Tuálati before mentioned, Mr. Gatschet also collected vocabularies and sentences of the following languages: Shoshoni, Achomáwi, Shasta, Wintun, Waccanéssisi, Waska, Klákamas, Mólele, Nestucca, Yamhill, Lukamáyuk and Ahantchuyuk. In the collection of all these vocabularies, the "Introduction to the Study of Indian Languages," prepared for the Smithsonian Institution by Prof. J. W. Powell was used.

Dr. H. C. Yarrow, U. S. A., now on duty at the Army Medical Museum in Washington, has been engaged during the past year in the collection of material for a monograph on the customs and rites practised in the disposal of the dead among the

North American Indians. To aid him in this work, circulars of inquiry have been widely distributed among ethnologists and other scholars throughout North America, and much material has been obtained which will greatly supplement his own extended observations and researches.

During the summer some interesting work was done in the examination of the stone graves of Tennessee, and valuable collections were made. Professor Powell has coöperated with the Institution in providing for a more thorough examination of the archæology of the islands off the shore of southern California. This exploration was made by Rev. Stephen Bowers, of Indianapolis, Indiana, and his report will be published with the papers of the survey.

A small volume, entitled "Introduction to the Study of Indian Languages," has been prepared. This book is intended for distribution among collectors. In its preparation, Prof. Powell was assisted by Prof. W. D. Whitney, the distinguished philologist of Yale College, in that part relating to the representation of the sounds of Indian languages. A few preliminary copies have been printed and distributed among gentlemen interested in the study of Indian languages for such addition and emendations as may be suggested preparatory to final publication. A tentative classification of the linguistic families of the Indians of the United States has been prepared. This will be published as soon as the bibliography is ready.

In pursuing these ethnographic investigations it has been the endeavor, as far as possible, to produce results that would be of practical value in the administration of Indian affairs, and for this purpose especial attention has been paid to statistics, the progress made by the Indians towards civilization, and the causes and remedies for the inevitable conflict that arises from the spread of civilization over a region inhabited by savages. It is believed that the labors in this direction will not be void of useful results.

Survey of the Black Hills.—In 1875, a reconnaissance survey was made of the Black Hills of Dakota, by Mr. Walter P. Jenny, with a corps of assistants, under the direction of the honorable Secretary of the Interior. On the return of the party from the field, Mr. Jenny's report relating to the mineral resources of the country was immediately published, but the geographical and geological report was unfinished at that time. This latter work was left in the hands of Mr. Henry A. Newton, his geological assistant, to be completed. On May 28th, 1877, at the request of Mr. Newton, the completion of the work was placed under the direction of this Survey by order of the Secretary of the Interior.

On consultation with Mr. Newton, it appeared wise that he

should visit the field again for the purpose of determining certain doubtful points in the geological structure, and to insert on the maps the position of the several towns and roads established in that region since the discovery of gold, and Mr. Newton was employed for this purpose. He had been in the field but a short time when he was prostrated by the sickness which resulted in his death. Previous to his departure he completed his report on the geology of that country, and the map had been placed in the hands of the engraver; the whole embodying all the facts discovered up to that time. Thus, happily, his work will not be lost. It is expected that his report will be published during the present winter, in the shape in which it was left by him.

The death of Mr. Newton makes a serious break in the ranks of the younger and more active geologists of America. He possessed rare abilities, had much experience in field operations, and had received thorough and wise training, and his work in other fields had exhibited his ability. But the great work of his short life will doubtless be his report on the geology of the Black Hills of Dakota.

During the past six years one branch of the work of the survey has been considered of paramount importance, namely, the classification of lands and the subjects connected therewith. The object has been to determine the extent of irrigable lands, timber lands, pasturage lands, coal lands and mineral lands. In general the lands that are cultivable only through irrigation are limited by the supply of water. There are some exceptions to this. Where streams are found in narrow valleys or run in deep cañons, the limit of agricultural land is determined by the extent of the areas to which the water can be conducted with proper engineering skill. In the study of this subject many interesting and important problems have arisen, and many valuable facts have been collected.

From the survey of the timber lands one very important fact appears, that the area where standing timber is actually found is very much smaller than the areas where the conditions of physical geography are such that timber should be found as a spontaneous growth—that is, the *area of timber* is but a small fraction of the *timber region*. The destruction of timber in such regions now found naked, is due to the great fires that so frequently devastate these lands; and the amount of timber taken for economic purposes bears but an exceedingly small ratio to the amount so destroyed. Hence the important problem to be solved is the best method by which these fires can be prevented.

Another subject which has received much attention is the utilization of the pasturage lands; and still another, the best methods of surveying the mineral lands for the purpose of description and identification, that the owners of mines may be relieved of the great burden of litigation to which they are subjected by reason of the inaccurate and expensive methods now in vogue.

ART. LXVI.—*Observations on the Transit of Mercury.* Letter to the Editors from JOHN RODGERS, Rear Admiral U. S. N., Superintendent of the United States Naval Observatory, dated May 11, 1878.

IT may interest your readers to learn that the transit of Mercury, occurring on May 5–6, was very successfully observed at the Naval Observatory, and throughout the country generally. Satisfactory observations of all the contacts were made here, and good observations of the contacts have been reported from the observers of the United States Coast Survey in Washington, and in different parts of the country. Reports of observations of the contacts have been received from the French astronomers at Ogden, Utah, and from the astronomers at Cincinnati, Chicago, Glasgow, Mo., and from amateur observers at New Orleans, Savannah, and various other points; so that we have an abundance of observations of this kind.

Since, however, observations of the contacts give only the correction of the longitude of the planet, the larger part of the appropriation of $1,500 made by Congress for observing this transit was expended in photography. The directors of the observatories at Cambridge and at Ann Arbor, Professors Pickering and Watson, kindly undertook the work of making photographs at their observatories; and a set of the instruments used in photographing the transit of Venus in 1874, was sent to each of these observatories. The method of taking the photographs was the same as that followed in the case of the transit of Venus. The dry-plate process was, however, adopted in the present case, in place of the wet-plate process used in the transit of Venus. The great advantage of the dry-plate process, if it can be used successfully, is evident. The plates were all prepared here by Mr Joseph A. Rogers; and seventy-two plates were sent to each of the observatories, where they were exposed and then returned here for development. The same number of plates were exposed here by Mr. Rogers.

The day at Washington was very favorable, and the plates were exposed here in groups of six, along the chord described by the planet across the disc of the sun. The development shows that all the photographs taken here can be measured accurately; and we think they will furnish data for a very exact determination of the latitude and longitude of the planet on the day of its transit.

The day at Cambridge was not favorable, and the development of the plates exposed there gives very thin photographs; some of which, however, can be measured. At Ann Arbor the day was cloudy during the latter part of the transit, and the plates were all exposed in the earlier part of the day. These photographs are all very dense, but probably all can be measured. Measurements of these photographs will be made as soon as possible and the results published.

A set of the transit of Venus photographic apparatus was furnished the French astronomers, Messrs. André and Angot, at Ogden, for the purpose of comparing the different methods of photographing the transit. These gentlemen report a snow storm during the early part of the day, but clear weather in the afternoon, during which a good number of photographs was secured.

Our experience in photographing transits of planets, and in measuring the photographs, indicates that while the American method is correct in theory, the apparatus needs some change. In order to obtain good measures, the picture should be sharp, and the exposure short. It is probable, therefore, that the reflectors, which now lose about nineteen-twentieths of the light, will have to be changed.

A comparison of the observations of contact with the ephemerides of the American and English Nautical Almanacs shows that the English Almanac is much nearer the truth. Since the ephemeris of the American Almanac is based on Leverrier's old theory, and that of the English Almanac on his recent one, the result of the present observations appears to be a confirmation of Leverrier's theory with respect to an intra-mercurial planet.

United States Naval Observatory, Washington.

Art. LXVII.—*Transit of Mercury of May 6th*, 1878; by S. P. Langley.

The following observations made during the transit of Mercury, yesterday, have some interest from the inferences to be drawn from them as to physical phenomena; and to devote the opportunity more wholly to this object, no measures of precision were attempted, beyond noting the times of ingress and egress. The principal instruments were the equatoreal refractor of thirteen inches (used in the early part of the day with nine inches effective aperture), and a polarizing solar eye-piece, which dispenses altogether with any dark glass, and presents objects in their natural colors and relative brightness.

I had the fortune, at ingress, of an unusually blue and transparent sky, and aided by this, saw with the polarizing eye-piece the entire disc of Mercury *outside* the sun about one-half a minute before first external contact. Presumably it might have been seen even earlier, had not time been lost in searching for it, through lack of means to designate the precise position-angle, the position filar-micrometer not being adaptable to this eye-piece. After a pause to verify the reality of the phenomenon by revolving the eye-lens, etc., the chronograph key was struck at $21^h\ 52^m\ 39^s{\cdot}45$ Allegheny mean time, to record the observation. As this was really made earlier, and the disc was seen throughout its circumference, it seems clear that the coronal back-ground is bright enough to produce this effect at least fifteen seconds of arc from the solar limb, and in spite of the atmospheric glare.

As a partial substitute for the filar micrometer, there was in the field a glass reticule, ruled (by Prof. Rogers, of Harvard) in squares whose sides represented here $15{\cdot}''3$, and this enabled —not a measurement—but a fair comparison to be made of the apparent size of the planet before and after it entered on the sun. The contrast was striking, as on a back-ground very little brighter than itself its diameter was, if anything, greater than one of the sides of these squares, while as soon as it entered on the sun it seemed to shrink by more than one-fifth of this. First external contact was noted on the chronograph at $21^h\ 52^m\ 50^s{\cdot}43$. First internal contact was noted when the sunlight could be seen unmistakably between the disc and limb at $21^h\ 55^m\ 47^s{\cdot}25$. These entries, I believe to have been made in both cases nearly two seconds late. The limb just at second contact, was steady. I saw no "black drop" or "ligament."

As the disc advanced on the sun it was closely scrutinized, without at any time any "bright point" or "annulus" being

seen. These appearances, resting as they do on much testimony, particularly the unimpeachable evidence of Mr. Huggins, I was prepared to expect, but fruitlessly looked for with powers varying from 120 to 800 throughout the day, with the polarizing eye-piece, and also by projection of the image. The phenomenon may depend for its visibility on exceptionally good definition, which Mr. Huggins* appears to have had; that here was fairly, though not unusually, good. The darkest part of the planet was the center, the edges being decidedly less gray. The cause of this gradation came out very clearly in forming a very enlarged image for projection, being plainly due in most part to minute and rapid atmospheric tremor. In moments of best definition the surface became of a nearly uniform shade throughout.†

The planet has been almost uniformly described as looking "black" in transit, but in the instrument I use (the objective of which was corrected by Mr. Alvan Clark), it certainly does *not* look black. The color is decidedly less red than that of spot nuclei, being gray, slightly inclining toward a blue, like that of the spectrum between F. γ. G. (It may be that this bluish cast comes from the secondary spectrum of the objective). The average light from the disc in transit is very considerable, being not much less than that of some nuclei. No spots were present for comparison, but being engaged in photometric determinations of these and other parts of the solar surface, I was provided with means of comparing Mercury with tints which had previously been contrasted with sun-spots under like conditions. Absolute photometric determinations of the apparent light from Mercury in transit were attempted by projecting a greatly enlarged image (its actual diameter was three-quarters inch as projected), on a white surface in a dark camera attached to, and moving with, the equatoreal. Direct measurements with a Jamin photometer were unsatisfactory. Subsequently, by another method, a trustworthy value was fixed for a minimum. It was thus found that the light actually received on the paper apparently from the so-called "black" body of the planet, at any rate exceeded eight per cent of that from direct sunlight, and measures taken by the thermopile and galvanometer showed that heat was coming from the same direction.

It need hardly be said that it is impossible that Mercury itself should be radiating heat and light in any such degree. Accordingly I take these numbers as representing (with some possible allowance for instrumental causes) the minimum effect

* Monthly Notices R. A. S., vol. xxix, p. 26.

† I presume that even in absolutely perfect definition there would be theoretically a slight gradation due to another cause, i. e., to the greater effect of the edge of the planet's disc of the inflection, referred to in a subsequent paragraph.

we can assign to our own atmosphere in inflecting the solar radiation, a subject on which data have been hitherto desirable. It is evident, for instance, that from the facts here stated we can estimate, photometrically, the intrinsic brightness of the corona, since it was undoubtedly this, acting as a back-ground, which enabled the planet, though itself involved to a calculable extent in atmospheric glare, to be seen before it reached the solar limb.

The observations were interrupted by haze in the afternoon and egress was so nearly invisible that the apparent times of contact are not worth giving.

Allegheny Observatory, May 7, 1878.

ART. LXVIII.—*Fossil Mammal from the Jurassic of the Rocky Mountains;* by Professor O. C. MARSH.

ONE of the most interesting discoveries made in the Rocky Mountain region is the right lower jaw of a small mammal recently received at the Yale College Museum. The specimen was found in the Atlantosaurus beds of the Upper Jurassic, and the associated fossils are mainly Dinosaurs.

Dryolestes priscus, gen. et sp. nov.

This specimen is in fair preservation, although most of the teeth have been broken off in removing it from the rock. The penultimate molar, however, remains. The shape of the jaw, and the position and character of the teeth, show that the animal was a small marsupial, allied to the existing Opossums (*Didelphidæ*). The tooth preserved has the same general form as the corresponding molar of *Chironectes variegatus* Illiger. The angle of the jaw is imperfect, but there are indications that it was inflected.

The principal dimensions of this specimen are as follows:

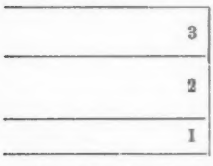
Space occupied by seven posterior teeth	12·5^{mm}
Depth of jaw below last molar	4·4
Transverse diameter	1·8
Height of crown of penultimate molar	2·
Transverse diameter	1·5

The present specimen indicates an animal about as large as a weasel. It is of special interest, as hitherto no Jurassic mammals have been found in this country.

Yale College, New Haven, May 13, 1878.

ART. LXIX.—*On some dark Shale recently discovered below the Devonian Limestones at Independence, Iowa; with notice of the Fossils at present known to be in it;* by S. CALVIN, Professor of Geology, State University of Iowa.

THE Devonian deposits of Iowa, as now known, may be roughly represented by the annexed diagram, in which 1 indicates the position of a member of the group recently discovered at Independence, consisting of dark argillaceous shales with some thin beds of impure, concretionary limestone. It has been explored to a depth of twenty or twenty-five feet. No. 2 represents all of what is usually included under the head of the Devonian limestones of Iowa, and is made up largely of limestone with some associated beds of light-colored shales; estimated thickness, 150 feet. No. 3 is a bed of argillaceous shales exposed at and near Rockford, Iowa, and is referred to frequently as the Rockford Shales. It abounds in fossils, and weathers, on exposure, into a stiff clay that has been utilized in the manufacture of brick; observed thickness, seventy feet.



Until recently, Nos. 2 and 3 of the above section were supposed to make up the entire thickness of Devonian rocks in Iowa. No. 2 not only varies, as already indicated, in lithological characters, but the grouping of fossils differs widely in different localities, so much so that competent geologists have referred certain exposures—for example those at Waterloo—to the Corniferous, and others—as at Independence and Waverly—to the Hamilton. Such reference of the above-named exposures will be found in the Twenty-third Report on the State Cabinet of New York, pp. 223–226, and in the same article Professors Hall and Whitfield declare the Rockford Shales to be the equivalent of the New York Chemung. On the other hand, Dr. C. A. White, Geology of Iowa, 1870, vol. i, p. 187, is of opinion that all the Devonian strata of Iowa belong to a single epoch.

Thus matters stood until about a year or so ago, when D. S. Deering called attention to the interesting fact that a dark shale had been exposed in working out the layers in the bottom of one of the limestone quarries near Independence. The quarrymen penetrated the shale to a considerable depth in the hope of finding coal. The shale varies somewhat, lithologically, but where it presents its most characteristic features, it is argillaceous, fine-grained and charged with bituminous matter. In some of the beds there are numerous remains of plants—stems of Lepidodendrons and Sigillarias that made up

the forests of the Devonian. The plants, however, are very imperfect; the form only is partially preserved, and that mainly by pyrite that replaced the original stem. The woody tissue of the plants has been in part converted into coal that occupies thin irregular seams among the laminæ of pyrite. The little bands of coal vary in thickness, but none of them observed exceed a quarter of an inch. None of the plants are perfect enough to render identification or description possible.

The discovery of shale charged with the carbonized stems of plants, below the Devonian limestones of Iowa, is a matter of much interest. Frequent reports have gained circulation of the discovery of coal in drilling wells in regions occupied by Devonian rocks. From Jesup, Janesville, Marion, Davenport and other places, such rumors have gone out. In two or three cases shafts were dug at considerable expense, necessarily ending in disappointment and failure.

The discovery at Independence accounts for these reports. In drilling through the limestones, the lower shale, with its carbonized plants, was reached, and the color of the borings mixed with fragments of real coal naturally enough gave rise to the impression that a veritable coal mine had been found.

It is to be noticed that all the places from which such reports have come stand near the eastern outcrop of the Devonian, where its entire thickness could be pierced at a very moderate depth. The number and position of such localities would show that the shale in question is not a mere local deposit, but is distributed all along the outcrop of Devonian rocks in Iowa.

The researches of Mr. Deering and myself have brought to light quite a number of finely preserved Brachiopods, representing fourteen different species. Of these, two are not determined and five others are new to science, but the chief interest attaches to certain species that have hitherto been known only from the shales of bed No. 3, near Rockford, Iowa. For the purpose of indicating the relationship of the new shale to the other Devonian deposits of Iowa, we shall arrange the specimens in three groups, using my manuscript names for the new species.

I. Species limited in Iowa as far as known, to the Independence Shales: *Strophodonta variabilis*, n. sp., *Gypidula munda*, n. sp., *Orthis infera*, n. sp., *Rhynchonella ambigua*, n. sp., *Spirifera subumbona* Hall.

II. Species ranging through all the Devonian deposits, and so common to beds 1, 2 and 3: *Atrypa reticularis* Lin.

III. Species common to beds 1 and 3, but not known to occur in the intervening limestones: *Strophodonta quadrata*, n. sp., *S. arcuata* Hall, *S. Canace* Hall and Whitfield, *S. reversa* Hall,

Atrypa hystrix Hall,* and *Productus* (*Productella*) *dissimilis* Hall.

It is an interesting fact that of the twelve determined species, six occur only in the shaly deposits at the opening and close of the Devonian, notwithstanding these deposits are separated by 150 feet of limestones. Only one species is known to pass from the lower shale into the limestone above, and even that appears under a form so altered that specimens from the two beds may be distinguished as readily as if they were distinct species. If we take form and surface markings into account the *Atrypa reticularis* of No. 1, also finds its nearest representative, not in the limestones immediately above, but in the shales at Rockford.

Obviously then the Independence Shales are more nearly related to the Rockford beds than to any other formation in Iowa. The species in group III seem to have disappeared with the ushering in of conditions under which limestones were formed; they maintained themselves in some locality which has not been discovered or from which the shaly deposits have been entirely swept away and returned with the return of conditions more favorable to them during the deposition of the Rockford Shales.

The intimate relation between the two extremes of the group can but strengthen the conclusion of Dr. White, that all the Devonian strata of Iowa belong to a single epoch.

Joseph Henry, LL.D.

Professor Henry died, on the thirteenth of May, 1878, at his home in the Smithsonian Institution, Washington City. For over half a century Professor Henry has been one of the foremost men of science in the United States; and his name is well known in all countries where science is cultivated. He was, it is believed, the last of that band of the older men of science in America, dating from the last century, having been the associate during his career, of Hare, Silliman, Bache, Torrey, and others of the same epoch. His eminent attainments and important discoveries early gave him a well earned reputation as an original investigator. Later his skill as an

* The form designated here as *A. hystrix* Hall, differs conspicuously from the form described in the Geology of Iowa, 1858, vol. i, part 2, p. 515, under the name *A. aspera* var. *occidentalis*. This last form is very abundant in the limestones at Independence. The specimens from the Independence Shale are identical with the form presented by this Atrypa in the Rockford shales. For application of this specific name to this special form, see Twenty-third Annual Report of Board of Regents on New York State Cabinet, p. 225.

administrator of great public trusts in the interests of science, and his rare personal qualities, made him universally respected and beloved. His noble presence and cultivated bearing were always conspicuous, however distinguished the society about him, and manifested truly the high *morale* which gave him a dignity rarely equalled. His genial smile, not unmingled with a certain reserve, will never be forgotten by those who enjoyed his friendship or came into familiar contact with him. Fortunately, a fine portrait painted quite recently, by order of the trustees of the Smithsonian Institution, will preserve his well known features and render them familiar to posterity.

Professor Henry was born December 17, 1797, at Albany, New York, where also much of his early life was passed. He had at first the advantages of only a common school education; later, after two years of work as a watchmaker, he came under the training of the Albany Academy, where he developed a degree of mathematical talent which, in 1826, led to his selection for the duties of instructor in mathematics in that institution. Prior to this, having had some experience in the field as a surveyor, he was associated with Amos Eaton in the Geological Survey along the line of the Erie canal, projected and sustained by General Stephen Van Rensselaer.

While occupied with his duties as mathematical instructor in the academy—then in charge of Dr. T. Romeyn Beck—he commenced that line of investigation in electricity which resulted in the important discoveries that have made his name famous. He attended the lectures of Dr. Beck on chemistry, and assisted in the preparation of his experiments. At this time he devised and published an improved form of Wollaston's sliding scale of chemical equivalents, in which hydrogen was adopted as the radix—a contrivance which is hardly known, even by name, to the present generation of chemists. Thus, while Professor Henry's original contributions to science were chiefly physical, his first scientific work was in the department of chemistry. His work with Dr. Beck enabled him, after his removal to Princeton—where he became Professor of Natural Philosophy in 1832,—to take up the duties of the chemist, Dr. John Torrey, when that well known teacher was disabled for a time by ill health.

It was in the interval, between 1828 and 1837, that the most important work of his life was accomplished in the line of strictly scientific research. These results are chiefly recorded in the Transactions of the Albany Institute, the volumes of this Journal for the period, and the Transactions of the American Philosophical Society. His "Contributions to Electricity and Magnetism," were collected in a separate volume in 1839. The analysis of these important researches, and the discussion

of the questions of priority connected with them, will be the duty of the academician to whom shall be assigned the preparation of a memoir or eulogy of the distinguished author.

Without assigning dates we give the following brief enumeration of his memoirs and discoveries, taken from a communication made by him to Prof. Silliman in August, 1874, for the purpose of insertion in an address delivered at Northumberland, Pennsylvania, on the occasion of the celebration of the centennial of chemistry. The order and enumeration are Professor Henry's:

1. A sketch of the topography of the State of New York, embodying the results of the survey before mentioned.

2. In connection with Dr. Beck and the Hon. Simeon De Witt, the organization of the meteorological system of the State of New York.

3. The development, for the first time, of magnetic power, sufficient to sustain tons in weight, in soft iron, by a comparatively feeble galvanic current.

4. The first application of electro-magnetism as a power, to produce continued motion in a machine.

5. An exposition of the method by which electro-magnetism might be employed in transmitting power to a distance, and the demonstration of the practicability of an electro-magnetic telegraph, which, without these discoveries, was impossible.

6. The discovery of the induction of an electrical current in a long wire upon itself, or the means of increasing the intensity of a current by the use of a spiral conductor.

7. The method of inducing a current of quantity from one of intensity, and *vice versa*.

8. The discovery of currents of induction of different orders, and of the neutralization of the induction by the interposition of plates of metal.

9. The discovery that the discharge of a Leyden jar consists of a series of oscillations backward and forward until equilibrium is restored.

10. The induction of a current of electricity from lightning at a great distance, and proof that the discharge from a thunder-cloud also consists of a series of oscillations.

11. The oscillating condition of a lightning-rod while transmitting a discharge of electricity from the clouds, causing it, though in perfect connection with the earth, to emit sparks of sufficient intensity to ignite combustible substances.

12. Investigations on molecular attraction, as exhibited in liquids, and in yielding and rigid solids, and an exposition of the theory of soap bubbles. [These originated from his being called upon to investigate the causes of the bursting of the great gun on the United States Steamer Princeton.]

13. Original experiments on and exposition of the principles of acoustics, as applied to churches and other public buildings.

14. Experiments on various instruments to be used as fog signals.

15. A series of experiments on various illuminating materials for light-house use, and the introduction of lard oil for lighting the coasts of the United States. This and the preceding in his office of Chairman of the Committee on Experiments of the Light-House Board.

16. Experiments on heat, in which the radiation from clouds and animals in distant fields was indicated by the thermo-electrical apparatus applied to a reflecting telescope.

17. Observations on the comparative temperature of the sun-spots, and also of different portions of the sun's disk. In these experiments he was assisted by Professor Alexander.

18. Proof that the radiant heat from a feebly luminous flame is also feeble, and that the increase of radiant light, by the introduction of a solid substance into the flame of the compound blowpipe, is accompanied with an equivalent radiation of heat, and also that the increase of light and radiant heat in a flame of hydrogen, by the introduction of a solid substance, is attended with a diminution in the heating power of the flame itself.

19. The reflection of heat from concave mirrors of ice, and its application to the source of the heat derived from the moon.

20. Observations in connection with Professor Alexander, on the red flames on the border of the sun, as observed in the annular eclipse of 1838.

21. Experiments on the phosphorogenic ray of the sun, from which it is shown that this emanation is polarizable and refrangible, according to the same laws which govern light.

22. On the penetration of the more fusible metals into those less readily melted, while in a solid state.

The following paragraph, from a letter of Mr. P. C. VanWyck published recently in the New York Times, has a special interest in this place, although repeating well-known facts, since it comes from one of his pupils, a graduate of Princeton of 1845.

"While attending Professor Henry's lectures the Professor was in the habit of demonstrating to the Senior Class the practicability of transmitting messages over a wire stretched from his lecture-room across the campus to his own study, and receiving answers back from Mrs. Henry—elementary, to be sure, yet nevertheless sufficient to demonstrate the principle. So, also, while lecturing on the subject of 'Sound,' he would have a long pole about an inch in diameter passing from the basement to the philosophical hall in the attic, on which was screwed firmly a rude imitation of a fiddle at the top near the ceiling, while to the other end negro Sam had a real fiddle attached.

On a bell signal from the Professor, Sam would saw away with his bow in the cellar, the Professor calling the attention of the class to the weird music his fiddle discoursed in the lecture-room. On these occasions Professor Henry always remarked that the function of the philosopher ceased when he demonstrated the principles of nature in his discoveries; that it then fell to the share of the inventor, by ingenious devices, to subordinate them to the uses of man."

Dr. Barnard's notice of his life and labors, in Johnson's Cyclopædia, is known to have been revised by Professor Henry. This fact is important in view of the interest attached to the question of priority in the discovery of the principles on which the electro-magnetic telegraph was invented. Henry's discovery in 1830 or 1831 of the production of powerful magnetism, *at a distance*, by the use of an intensity battery current and a reduplication of the windings, in separate short circuits, of conducting wire, and the use of an automatic circuit breaker to sound a bell, anticipates by many years any electro-magnetic telegraph. It is, in fact, in universal use to-day; for all skilled operators read by sound, the record on a paper being practically of little use.

To the above enumerated papers should be added an important series of communications, made chiefly to the National Academy of Sciences during the past four or five years, upon the laws of acoustics as developed in the course of investigations conducted for the Light-House Service in order to determine the various conditions involved in the transmission of fog-signals. These investigations have been carried forward mainly in government vessels, and occupied Professor Henry's close personal attention during many weeks of each season.

Besides these experimental additions to physical science, Professor Henry is the author of thirty reports, between the years 1846 and 1876, giving an exposition of the annual operations of the Smithsonian Institution. He has also published a series of essays on meteorology in the Patent Office Reports, which, along with an exposition of established principles, contain many new suggestions; and, among others, the origin of the development of electricity, as exhibited in the thunder-storm.

Professor Henry remained at Princeton, in the chair of Natural Philosophy until his removal to Washington in 1846, to enter upon the duties of Secretary of the Smithsonian Institution.

Favored by nature with a vigorous constitution, he enjoyed through his long life almost uninterrupted good health. His powerful frame and calm spirit enabled him to sustain with comparative ease the pressure of an uncommon burden of official labor which fell to his share as the head of the Smithsonian Institution; of the Light-House Board; of the National

Academy of Science; and as the adviser of the Government in matters of science.

It was while engaged in discharge of certain experimental work on Staten Island last December, connected with the photometric laboratory of the Light-House Board, that he experienced a partial paralysis, which yielded soon to treatment, but was doubtless the precursor of the nephritic attack to which he succumbed. In April he presided at the opening meeting of the session of the National Academy of Sciences held in the rooms of the Secretary of the Smithsonian, and submitted an address to his associates, read by the Home Secretary, recounting with touching simplicity his recent decline of power, and expressing his desire to be relieved from the cares of the office of President. As a mark of affectionate respect, the Academy unanimously requested him to retain this post during his life—leaving the duties to be discharged by the Vice-President. It was on this occasion that the announcement was made to the Academy, by Professor Henry, and, subsequently, in fuller details, by Professor Fairman Rogers, the Treasurer, of the creation of an endowment to be called "the Joseph Henry fund." This fund consists of forty thousand dollars, securely invested, the income of which is for the support of Professor Henry and that of his family, during the life of the latest survivor. Afterwards the fund is to be transferred, in trust, to the National Academy of Sciences, the income to be forever devoted to scientific research. No more graceful and well-merited tribute of respect and affection was ever bestowed upon a man of science, by the spontaneous offerings of personal friends and associates. Alas! that its honored object should have remained so brief a time to enjoy the peace and satisfaction of this gracious endowment. If it is true that Republics are ungrateful, it is pleasant to know that the absence of imperial and kingly patronage may be compensated by a sovereignty not less potent.

It is a truly fortunate circumstance for the science of this country that a man of Professor Henry's pure and exalted character was so long in a position of such influence in public affairs at the seat of government. By force of his earnest determination that the will of the Testator should be carried out, he has saved the Smithson fund from diversion and absorption in a public library, and from various other schemes, and has succeeded in devoting it "to the increase and diffusion of knowledge among mankind," to cite the words used in Smithson's will. By his prudent management of the finances, this fund, after paying for the building an amount nearly equal to the original fund, and certain losses incident to the ordinary chances of investments, is larger to day by one-half than the original fund paid over to the United States. The policy upon

which the Smithsonian Institution was instituted and has been carried forward, was mainly the result of the united efforts of Professor Henry and his friend, and able counsellor, the late Professor Alexander D. Bache, whose skill and success as an administrator stood the test of a long and successful career, at Washington, as the head of the United States Coast Survey, from 1845 to 1863. The history of the successive struggles by which the present organization of the Smithsonian Institution was brought about, will be found in the successive annual reports. That this long period of activity, over thirty years, devoted so largely to work almost purely administrative, was a severe tax upon a man of Professor Henry's great productive power and ability in original research can hardly be questioned. On the other hand, it rarely falls to the lot of any man of science to do so much for the best interests of the entire body of scientific workers, or to succeed so well in securing respect and confidence on the part of the public for scientific results and methods. This is conspicuous in many ways in the history of the Smithsonian Institution, and in nothing more perhaps than in the direction which has been given to the numerous governmental explorations during the past twenty years, under its guidance.

Professor Henry leaves a wife and three unmarried daughters, who have been assiduous helpers in the scientific work of their father, making good to a degree the loss of an only son, whose death in early manhood, was a sad disappointment of parental hopes and youthful promise.

Professor Henry was buried May 16th, in the Rock Creek Cemetery, near Georgetown, D. C. The President of the United States, the cabinet officers, diplomatic corps and members of Congress and of the National Academy, were among the mourners. B. S.

ART. LXXI.—*Letter to the Editors from* DR. B. A. GOULD, *of the Cordoba Observatory, dated Cordoba, March* 20, 1878.

I HAVE before me the first part of three different letters to you, which I have begun during the last eighteen months, but the ceaseless pressure of labor in collecting and arranging materials during this interval has left little opportunity for elaborating, and none for communicating them. At last this phase of the work is drawing to a close, and the objects which I had in view in the beginning may be regarded as essentially fulfilled so far as observations are concerned. The part which remains is but the computation and publication of these results, and is going forward as rapidly as the nature of the case permits.

The climate of Cordoba, which during the first years of my sojourn was peculiarly favorable for astronomical work, has showed itself variable to a remarkable degree. The routine of work with the meridian circle goes forward, however cloudy the sky, provided even a small part of the stars desired can be seen through the haze or between the clouds, and very often the thunder is rolling and lightning flashing over a third part of the sky while observations are making on the meridian. Yet in the last year no observations were possible on more than half the nights in March and April, and only ten nights in these two months could be regarded as clear. In June and July the case was worse yet, for there were but seven nights which were clear, and on twenty-eight not a star could be descried from nightfall till dawn. This year the weather has been equally unfavorable, and during the seventy-nine days already elapsed, there has been but one night on which our observations have not been to some extent prevented or interrupted by clouds. The contrast between such condition of the weather, and that which prevailed some years ago is very striking, and seems to point to some periodic fluctuation.

I am all the more disposed to believe this, since arriving at the results relative to the periodicity of sundry phenomena which came to light in the study of the climate of Buenos Aires, as mentioned in my annual report for the year 1876. The volume containing these results, being the first of the Annals of the Meteorological Office, is already printed—the only one yet published of either the astronomical or meteorological series—and refers to the city of Buenos Aires alone. I hope it may soon reach you; but as the process of distribution is necessarily slow, and attended here with many obstacles, I will mention some of the results to which I allude, and which are there discussed in detail.

In the first place the difficulties which have attended previous attempts to determine the relation between the mean temperature and the number of sun-spots have been found to disappear when the local influences which affect the temperature are removed. I have not at hand the means of knowing what these influences may be in other places, but the thermic wind-rose has sufficed for Buenos Aires. Determining for each year the mean direction of the wind, and deducting its effects from the mean temperature as observed in the same year, there results a curve which follows that of the sun-spot so closely as scarcely to leave anything more to be desired. I have only used the mean of the wind-roses determined for ten days each in spring, summer, autumn and winter; nor has the mean velocity of the wind been brought into the computation. More thorough calculations based on a wind-rose more minutely determined and upon the consideration of the total amount of air of a different temperature flowing in, would doubtless increase the degree of agreement; yet the discordance between the mean temperature observed, and that which results from the empirical calculation does not amount in any one of the twenty years, 1856–1875, to so much as might fairly be attributed to errors of observation.

Thus if we denote by r the relative number corresponding to the mean amount of sun-spots, the normal mean temperature of Buenos Aires in centigrade degrees is $17°{\cdot}50 - 0°{\cdot}00727r$, so that 138 of Wolf's scale correspond approximately to 1° C. in the temperature. Thus after deducting the effect of the wind, I find the mean temperatures, as observed, to have been 16°·47 in 1870, and 17°·51 in 1875, while those which result from the formula are 16°·63 and 17°·49 respectively. The difference 0°·16 for the first named year is by far the greatest which presents itself in the whole series, the next largest one being 0°·10, and the mean discordance only 0°·056. The want of perfection in the wind-rose, and the excessive deflection of the mean direction of the wind in 1870 afford what seems to me a sufficient explanation of the relative largeness of the residual.

It is manifest that if the variations of the terrestrial temperature follow those of the sun-spots, and are thus adequate to account for the correspondence observed between these and the variations of the magnetic declination, all necessity for assuming any direct and transcendental connection between this latter and the disturbance of the solar surface disappears.

It is a source of regret that I have not at disposal here any series of mean annual temperatures observed in Europe or North America, accompanied by the corresponding mean direction of the wind; for I have small doubts that analogous results might be deduced, although it is possible that other local disturbances of the temperature may have to be taken into account beside the effect of the wind. There is one other point in South America for which the investigation may be made, viz: Bahia Blanca, just to the north of the Patagonian coast, and this I intend to investigate as soon as time and opportunity permit.

The mean annual direction of the wind varies through an arc of 60° during the twenty years mentioned, having been 70° in 1857, 77° in 1870, and 130° in 1874 (these angles being counted from the north point through the east). The accordance between its variations and those of the spots, which Mr. Main has found in Oxford, does not hold in Buenos Aires, but the study of the phenomenon has brought to light the existence of two cycles in the numbers which represent the mean annual value of $R \sin \varphi$, or in other words in that component of the mean annual direction which is perpendicular to the meridian. Investigated without any assumption or hypothesis whatsoever, they show two periodic terms of which the lengths are 2·78 and 3·70 years. These periods are respectively the quarter and the third part of 11·11 years, the mean length of the sun-spot period, and their existence opens a field for new inquiry.

Another and yet more marked cycle shows itself in the frequency of the storms, which are so well known and characteristic of the La Plata. Whether we consider the annual number of winds of force five, or six, or seven and upwards, or the mean annual force, the same result manifests itself, viz: a periodic

fluctuation, the maximum having been early in 1864, the minimum between 1871½ and 1876, and the duration between eighteen and twenty-three years. The form of the corresponding curve is such that it varies but slightly near the minimum, and the period is so nearly equal to that comprised in the series of observations, that it cannot yet be fixed with accuracy. But I am inclined to believe it will be found not to differ much from twenty-two years, in which event we have still another case of near commensurability with the mean period of the sun-spots.

As regards the astronomical work there is little to communicate as yet. The Uranometry is not yet published, and I am fearful that it was a mistake to undertake the printing in this country; not because it cannot be well and even elegantly accomplished, but on account of the long time which it requires, and the limited amount of type available. I am doing all that seems possible to remedy these difficulties, but it is of course at much expense of time and energy. The Atlas is completed; the numerous technical difficulties in the way of a satisfactory reproduction of the drawings having been obviated by the skill and assiduity of the photolithographer, Mr. Julius Bien of New York. My first assistant, Mr. John M. Thome, to whom more than to any other is due what accuracy the determinations of magnitude may possess, was in the United States during the past year, and personally attended to the proofs. He returned a few weeks since, bringing with him some advanced copies, with which I am quite satisfied.

The Atlas consists of thirteen charts, on the scale of a globe of one meter radius, and contains all stars to the 7·0 magnitude inclusive, within a radius of 100° around the South Pole. To these is added a fourteenth as a sort of a Index-Map, to show the limits of the individual charts, the course of the Milky Way, and the general distribution of the stars. This map of course comprises more than a hemisphere, and is projected on a scale of equal areas; the distances between the parallels of declination decreasing as those between the meridians of right ascension increase. The configurations are necessarily distorted, but the degree of aggregation of the stars is correctly given.

As mentioned in former letters I have ventured upon rather a bold reformation of the boundaries of the constellations, which I earnestly hope may find approval with astronomers generally. Wherever possible, meridians and parallels (equinox of 1875·0) have been employed as boundary lines, and in other cases great circles so far as might well be. Yet the principle has been sedulously followed that no important star, and none habitually designated by a Greek letter, should be transferred to a different constellation. By a slight sacrifice of this principle the symmetry of the adopted boundaries might have been essentially increased, yet I have preferred to err upon what seemed the safe side.

The text will be in English as well as Spanish, but the Latin names of the constellations have been preserved, as the only basis for international accordance in nomenclature.

Twice during the last year the normal clock of the observatory has been stopped by earthquake waves, although the tremors seem to have been felt in neither instance by persons in this vicinity. One of these cases was at the time of the great earthquake which destroyed the town of Iquique, and produced so much destruction along the coast of Peru, Bolivia and Northern Chile. The other was at the time of the severe shock at Mendoza. The moments at Cordoba were of course very accurately given by the clock itself, and accounts carefully obtained from the points of chief disturbance give the same result as on former occasions, viz: that the interval of time between the manifestations at these points and at Cordoba was less than the uncertainty of the watches. This you may remember was the case once before, when the shock was distinctly felt in Cordoba, although the clock was not stopped. The clock of the telegraph-station at Mendoza had been regulated from this observatory only a day or two previous, and the time as shown by the dial agreed with that of my watch in Cordoba. Whether the tremors traveled over the intervening distances or not, it seems at least demonstrated that the interval between their arrival at the two places is less than can be measured without special preparation and precautions.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *The Expansion of the Solid Elements a Function of their Atomic weight.*—Since all gases, under similar physical conditions, contain in equal volumes the same number of molecules, they must all have the same coefficient of expansion, because this expansion represents simply an increase in the energy of the progressive motion of the molecules. For the non-gaseous elements, however, no such simple relation has been hitherto observed. Wiebe has thrown some light upon the subject, by studying the expansion-coefficients of the solid elements in connection with their atomic weights. The quotient of the density of any such element, referred to water as unity, divided by its atomic weight, gives the space occupied by one atom of that element. If the ratio of this value to the coefficient of expansion be obtained, some striking relations appear, as is shown in tabular form for twenty-six solid elements. In the first column the symbols are given, in the second the density, in the third the atomic weight, in the fourth the expansion-coefficient expressed in parts in a hundred million at 40° C., and in the fifth the ratio of this to the quotient of the density divided by the atomic weight. The numbers in the fifth column then, represent the absolute expansion of the atoms, or the coefficients of expansion of the elements referred to their atomic volumes. These atomic coefficients often show simple relations for closely allied elements; thus the value is

the same for iron, cobalt and nickel, the numbers for arsenic, antimony and bismuth are as 1 : 3 : 4, those for zinc and cadmium as 2 : 3, etc. If these coefficients be regarded from the stand point of Lothar Meyer's law of the periodicity of the properties of the elements, and be graphically represented as a function of the atomic weight, they give a curve similar to that of the atomic volume, in which members of natural families have an analogous position. Hence the author concludes that the absolute expansion of the atom is a periodic function of its atomic weight.—*Ber. Berl. Chem. Ges.*, xi, 610, April, 1878. G. F. B.

2. *On the New Metal, Gallium.*—In connection with JUNGFLEISCH, LECOQ DE BOISBAUDRAN, the discoverer of gallium, has worked up the residues obtained from 4,300 kilograms of the Bensberg zinc blende, to obtain more of the new metal. The blende was first pulverized and then roasted, the product treated with sulphuric acid in quantity nearly sufficient to dissolve all the zinc, thus leaving a residue of basic sulphate containing the gallium, the residue dissolved in sulphuric acid, the iron reduced by zinc and the solution precipitated fractionally by sodium carbonate several times, the spectroscope being used to detect the metal. In this way, the gallium became concentrated in a residue weighing 100 kilograms. This was dissolved in sulphuric acid, purified with hydrogen sulphide, and treated with ammonium acetate, the H_2S being continued. The zinc sulphide thus precipitated, carried down the gallium. It was dissolved in sulphuric acid, fractionally precipitated by sodium carbonate, the fractions dissolved in the minimum quantity of sulphuric acid, precipitated by H_2S, diluted with water and boiled. A voluminous basic salt of gallium came down which after purification was dissolved in potassium hydrate, and the metal obtained directly from the solution by electrolysis, using forty Bunsen cells in eight parallel series of five each, the negative electrode having a surface of fifteen square centimeters and the positive of 450. In the cold, long crystalline metallic filaments stood out from the electrode, sometimes three centimeters in length. Above 30°, the metal ran in drops to the bottom of the electrode. In this way sixty-two grams of crude gallium were obtained. Taking the inevitable loss into account, the blende of Bensberg contains $\frac{1}{60000}$ of gallium or sixteen milligrams per kilogram. Purified by straining through a cloth, agitating with hydrochloric acid, and recrystallizing, it was obtained as a hard, slightly malleable metal, which can be extended under the hammer or bent rather abruptly, but is quite brittle. On cooling the fused metal to 10° or 15° below its fusing point, and introducing a platinum wire having a bit of solid gallium on one end, octahedral crystals are obtained, their summits modified by the basal planes. Gallium leaves a bluish gray mark on paper, is permanent in the air, remains brilliant even in boiling water, but tarnishes slightly in aerated water. In fusion it is white like tin or silver, but becomes blue-green on solidifying. Chlorine attacks it readily, evolving much heat and producing a well crystallized,

very fusible and volatile chloride, colorless when pure and deliquescent. Bromine acts less powerfully, giving rise to a white bromide, and iodine a white iodide, both of which resemble the chloride. The atomic weight the authors determined to be 69·9.

DUPRÉ, having received a portion of the metal, has undertaken a study of its salts. Heated to 260° in pure oxygen it undergoes no change. At a dull red, it begins to lose its luster and to be covered with a bluish gray pellicle. At a bright red, this pellicle is thicker, a feeble sublimate being formed. Treated with strong nitric acid at 40° or 50°, it dissolves; and the solution heated to 110°, loses nitric acid. Redissolved in water, evaporated to a syrupy consistence, and placed under a dessicator, the nitrate is obtained as a white very deliquescent mass, which may be dried in a current of hot air at 40°. The dry nitrate heated to 200° loses 63·8 per cent in weight, thus proving it to be a nitrate of the sesquioxide. At a higher temperature the nitrate melts, decomposes, and leaves a white friable mass, gallium sesquioxide. Heated to redness in a current of hydrogen, it partly sublimes and is partly reduced. At a cherry red, the mass is bluish gray, but contains no metal, since it dissolves in nitric and in sulphuric acid with no evolution of gas. Since the latter solution reduces permanganate, it is probably a protoxide salt; a view confirmed by the fact that it does not form an ammonium alum as does the sesquioxide sulphate. At a bright red heat, hydrogen reduces a portion of the oxide to the metallic state.—*C. R.*, lxxxvi, 475, 577, 720, 756, Feb., March, 1878. G. F. B.

3. *On Dimethyl-ethylene, or normal Butylene.*—The amyl alcohol of commerce, as is well known, is a mixture in variable proportions of active or ethyl-methyl-ethyl alcohol $\left.\begin{matrix}C_2H_5\\CH_3\end{matrix}\right\}CH-HH_2OH$ and inactive or isopropyl-ethyl alcohol $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}CH-CH_2-CH_2OH$. Dehydrated by zinc chloride in the ordinary way, it furnishes an amylene consisting of four different bodies: two soluble in sulphuric acid diluted with half its volume of water, ethyl-methyl-ethylene and trimethyl-ethylene; and two insoluble in this liquid, isopropyl-ethylene and normal amylene. In order to avoid polymerization in the preparation of amylene, ETARD has modified the ordinary process by placing the zinc chloride, about 500 grams, in a spacious metal retort, heating it in a gas furnace to full fusion and running in a thin stream of amyl alcohol, the vapors being condensed in a long worm. The amylene was about one-third of the entire product, boiled between 35° and 38°, was completely absorbed by bromine, and with the exception of 3 or 4 per cent, consisted of isopropyl-ethylene $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}CH-CH=CH_2$, produced from the corresponding alcohol. The associated ethyl-methyl-ethyl alcohol as well as the admixed propyl and butyl alcohols came over unchanged. But small quantities of polymers were produced, diamylene only being isolated.

Wishing to try this method with butyl alcohol, in order to obtain normal butylene, Le Bel and Greene allowed this alcohol to fall drop by drop upon zinc chloride heated to fusion in a mercury bottle, the products being collected first in a washing apparatus consisting of a flask, two Pettenkofer's tubes and a Liebig condenser, all containing diluted sulphuric acid to retain the isodimethyl-ethylene, and then passing through bromine. From a kilogram and a half of butyl alcohol, nearly a kilogram of bromide was obtained, which readily separated into three fractions, boiling at 145°–152°, 152°–154° and 154°–160°, the latter fraction forming three-fifths of the whole, and being $CH_3-CHBr-CHBr-CH_3$, mixed with traces of $CH_2Br-CBr-(CH_3)_2$. No ethyl-vinyl was formed in the reaction. Treated with sodium the reaction was violent, and the products collected in hydriodic acid yielded pure secondary butyl iodide, $CH_3-CHI-CH_2-CH_3$, thus proving the presence of normal butylene or dimethyl-ethylene in the above reaction of zinc chloride on butyl alcohol. There are then two normal butylenes, i. e., those in which the carbon atoms are united to no more than two others; one of these is ethyl-vinyl, $CH_3-CH_2-CH=CH_2$ and the other dimethyl-ethylene $CH_3-CH=CH-CH_3$. Upon the hypothesis that in uniting with a hydracid, the hydrogen and the haloid play the same part as the hydrogen in the saturated molecule and hence that all the hydrogen atoms attached to the same carbon atom have the same value, both these bodies should yield the same hydriodate, $CH_3-CH_2-CHI-CH_3$; while if the hydrogen of the HI has a different value, the two hydriodic compounds will be isomeric. Preparing carefully the two bodies they were found to be identical in properties, both boiling at 118°–121°, and both yielding a butylene by the action of alcoholic potash which gave a bromide distilling between 153° and 160°.—*C. R.*, lxxxvi, 488, Feb.; *Bull. Soc. Ch.*, II, xxix, 306, April, 1878. G. F. B.

4. *Chemical Composition of Oil of Tansy and Oil of Valerian.*—Bruylants has submitted to proximate analysis the oils of tansy and of valerian. The former is a mobile yellow liquid, of sp. gr. 0·923 at 15°, begins to boil at 192° and distils mostly between 194° and 207°, the thermometer rising finally to 270°–280°, leaving a resinous mass about a tenth of the whole. Treated with a concentrated solution of hydro-sodium sulphite and then alcohol, oily drops appear which become crystalline and fall to the bottom. They are pearly plates of tanacetyl-sodium sulphite, soluble in dilute alcohol, insoluble in benzene and ether. Heated with a dilute solution of sodium carbonate, the organic portion is obtained as an oil, of density 0·918 at 4°, remaining liquid at —15° and boiling at 195°–196°. Analysis gave the formula $C_{10}H_{16}O$, to which the vapor density 5·07 corresponds; it is thus an isomer of laurel camphor. By the action of H, the tanacetyl hydride gives an alcohol, $C_{10}H_{18}O$, by abstraction of H_2O, it gives cymene $C_{10}H_{14}$, by action of phosphoric chloride, tanacetene dichloride, tanacetene monochloride and cymene, and with ammo-

nio-silver nitrate it gives the mirror characteristic of an aldehyde. Oxidized with chromic acid it gives acetic and propionic acids, with nitric, it yields camphoric acid. The portion of the oil not acted on by the sulphite proved to be an alcohol $C_{10}H_{18}O$, with a small quantity of a hydrocarbon $C_{10}H_{16}$. Hence tansy oil consists of 70 per cent of an aldehyd $C_{10}H_{16}O$, of 26 per cent of an alcohol $C_{10}H_{18}O$, and of 1 per cent of a terpene $C_{10}H_{16}$. Valerian oil proved to be composed of: (1) a terpene $C_{10}H_{16}$, (2) an alcohol $C_{10}H_{18}O$, (3) borneol formate, acetate and valerianate, $C_{10}H_{17}.CHO_2$, $C_{10}H_{17}C_2H_3O_2$, $C_{10}H_{17}C_5H_8O_2$, and (4) ethylborneol oxide, $C_2H_5.O.C_{10}H_{17}$.—*Ber. Berl. Chem. Ges.*, xi, 449, March, 1878. G. F. B.

4. *Formation of Xanthin-like bodies from Albuminates in Pancreatic Digestion.*—SALOMON has succeeded by the action of the pancreatic ferment upon pure blood fibrin, in preparing hypoxanthin and very probably also xanthin itself. The former has been observed only as a product of putrefaction hitherto.—*Ber. Berl. Chem. Ges.*, xi, 574, April, 1878. G. F. B.

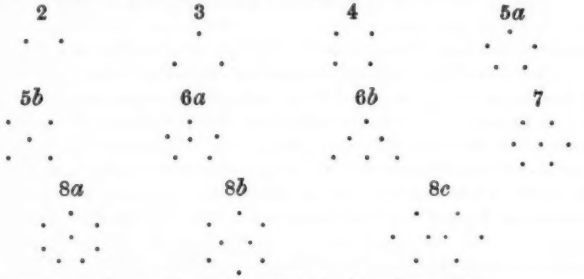
8. *The present Condition of Electrical Meteorology.*—PALMIERI, the Director of the observatory upon Vesuvius, has published in the Atti della R. Accad. di Napoli, VII, p. 1–20, 1877, a resumé of his observations upon the electricity of the air, which he has conducted during the past twenty-seven years. He also gives a description of the apparatus which he has found best suited to his purpose. The electrometer resembles, at first sight, that of Dellman; it differs, however, essentially from the latter. The suspension of the needle is bifilar and the repulsion between the fixed arm and the needle is not due to the repulsion of two bodies charged by conduction to the same amount, but is the result of induced charges. The needle is provided with a horizontal disc which is carefully centered over a vessel of which it is apparently the cover. This vessel is placed in communication with the testing conductor. The excursions of the point of the needle are read by means of a microscope. The chief peculiarity of the conductor consists in this, that it ends in a plate twenty-seven centimeters in diameter, and the connection with the electrometer is broken just as the conductor reaches the limit of the height to which it is raised, which is about 1·5 meters.

With feeble electrical condition the first swing of the needle is the double of its final deflection. With large differences of potential the fall of deflection is less. This diminution of indication gives a measure of the electrical disturbance. The deflections are compared with those produced by the poles of thirty CuZn elements. A Bohnenberger's electroscope serves as an indicator of the kind of electricity. The director believes that his instrument is preferable to Thomson's self-registering electrometer. The observations tend to show that the electricity of the air is +, and that a negative state of the electricity of the air is due to some local disturbance. The strength of the + electricity of the air increases with the relative moisture of the stratum of

air over the apparatus. Simultaneous observations upon the observatory of the University and at the Capodimonte in Naples, also at the observatory upon Vesuvius. More distant observations upon the little St. Bernard and in Moncalieri have impressed the author with the belief that with dry clear air, in which the distribution of + electricity is regular, the strength of the influence diminishes with increasing height.—*Beiblätter Physik und Chemie*, vol. ii, no. 3, p. 155. J. T.

10. *Floating Magnets.*—Nature, for May 2, contains a paper by Sir William Thomson, in which he observes that Professor Mayer's "beautiful experiment [this volume, p. 276], brings us very near an experimental solution of a problem which has for years been before me unsolved,—of vital importance in the theory of vortex atoms;" and also that the experiment had interested him particularly "because the mode of experimenting, with a slight modification, gives a perfect mechanical illustration (easily realized with satisfactory enough approximations) of the kinetic equilibrium of groups of columnar vortices revolving in circles round their common center of gravity, which formed the subject of a communication which I had made to the Royal Society of Edinburgh, on the previous Monday"—that preceding the republication of Professor Mayer's article in Nature—a point which the paper goes on to explain and illustrate.

11. *Note on Floating Magnets;* by Professor A. M. MAYER. (From a letter to the Editors, dated South Orange, New Jersey, May 21, 1878.)—I was much gratified to know that my experiments have proved of so much use to Sir William Thomson. When I received that number of Nature containing Professor Thomson's article on the floating magnets, I was engaged in writing a communication to him, giving him a concise statement of the morphological laws ruling the configuration of the floating magnets, with other new points of interest. These laws I discovered a week or two after I sent you the short note about these experiments, published on page 276. These laws are as follows: the configurations of the floating magnets are divided into primary, secondary, tertiary, quaternary, etc., classes, and the configurations of one class form the *nuclei* to the succeeding ones. The following are the primary configurations:



The configuration found of 9 magnets begins the secondaries; and this configuration has 2 for its nucleus. The secondaries have for nuclei the *stable* primaries, i. e., configurations numbered 2, 3, 4, 5*a*, 6*a*, 7 and 8*a*. The tertiaries have the secondaries for nuclei; the quaternaries, the tertiaries, etc.

If the configuration be made with the superposed magnet at a constant vertical distance, it will be found that when the same number of floating magnets form different configurations, that these different forms always have different *densities*, just as in the phenomena of allotropy, isomerism, etc. In configurations formed of the same number of needles the densities are necessarily inversely as the areas. These experiments beautifully illustrate this, that different crystalline forms, of the same chemical composition, depend on the different *directions* of molecular forces, which in all the forms may have the same intensity.

II. Geology and Mineralogy.

1. *Supplement to the Second Edition of Acadian Geology;* by J. W. Dawson, LL.D., F.R.S. 102 pp.*—This publication contains the new matter added to the third edition of "Acadian Geology," just issued; and which is published separately in this form for the benefit of those who already possess the second edition. It reviews the new facts which have been discovered in the Maritime Provinces of the Dominion of Canada since 1868. Beginning with the later deposits, the author endeavors to vindicate by new facts his former conclusion that the cold of the Glacial period was not connected with a continental glacier, but with local glaciers on the higher lands and ice-drift by Arctic currents over the plains, then submerged. He subdivides the Post-pliocene deposits as follows, in ascending order:

(*a.*) Peaty terrestrial surface anterior to bowlder clay.
(*b.*) Lower stratified gravels and sands.
(*c.*) Bowlder clay and unstratified sands with bowlders. Fauna, when present, extremely Arctic.
(*d.*) Lower Leda clay, with a limited number of highly Arctic shells, such as are now found only in permanently ice-laden seas.
(*e.*) Upper Leda clay and sand, or Uddevalla beds, holding many sub-Arctic or boreal shells similar to those of the Labrador coast at present.
(*f.*) Saxicava sand and gravel, either non-fossiliferous or with a few littoral shells, of boreal or Acadian types.

After some notice of the Trias, extensively developed in Prince Edward Island, where it has afforded the remains of one Dinosaurian Reptile and several land plants, and which in Western Nova Scotia is so remarkable for its great trappean beds, a large space is devoted to the Carboniferous, and more especially to the recognition of an Upper "Permo-carboniferous" or perhaps truly Per-

* New York: Van Nostrand. We are indebted for this notice to Dr. Dawson, the author of the work.

mian member, consisting principally of red sandstones, and holding a somewhat peculiar flora akin to that of the Lower Permian of Europe. Details and illustrations are also given of new species of Batrachians, Fishes, Insects, and Crustaceans, recently discovered, and an analysis and comparison with other countries, is made of the remarkable development of the Lower Carboniferous series of Nova Scotia and New Brunswick.

After a short notice of the Devonian, which, in the region referred to, is chiefly remarkable for its rich flora, in the main distinct from that of the Lower Carborniferous, and now numbering 125 described species, the author proceeds to discuss the difficulties attending the study of the Silurian and Cambrian formations, in a region where they are much disturbed and altered, and associated with igneous beds of very varied character. On this subject he remarks:

"In the Acadian Provinces, as in some other parts of Eastern America, the great igneous outbursts, evidenced by the masses and dykes of granite which cut the Lower Devonian rocks, make a strong line of distinction between the later and older Paleozoic. While the Carboniferous series is unaltered, except very locally, and comparatively little disturbed, and confined to the lower levels, the Upper Silurian, and all older series, have been folded and disturbed and profoundly altered, and constitute the hilly and broken parts of the country. Further, in the Upper Silurian and the older periods, there seems to have been a constant mixture with the aqueous sediments in process of deposition of both acidic and basic volcanic matter, in the form of ashes and fragments, as well as probably outflows of trachytic and dioritic rock, so that all these older formations are characterized by the presence of felsite and porphyry and petro-siliceous breccia, and of diorite. Further, since these volcanic and tufaceous rocks, owing to their composition, are much more liable to be rendered crystalline by metamorphism than the ordinary aqueous sediments from which the bases have been leached out by water, and since they are usually not fossiliferous, the appearance is presented of crystalline non-fossiliferous rocks alternating with others holding abundant organic remains, and comparatively unaltered. The volcanic members of these series are also often very irregular in distribution, and there is little to distinguish them from each other, even when their ages may be very different. These circumstances oppose many difficulties to the classification of all the pre-Devonian rocks of Nova Scotia and New Brunswick, difficulties as yet very imperfectly overcome."

In New Brunswick and in Eastern Maine, it appears that the fossiliferous Upper Silurian rocks are capped by felsites, chloritic schists and agglomerates of great thickness, and having an aspect not unlike that of the older Huronian, while in Eastern Nova Scotia similar rocks appear locally at the base of the Upper Silurian. Again, all the middle part of the Lower Silurian period seems to have been characterized by the deposition of similar vol-

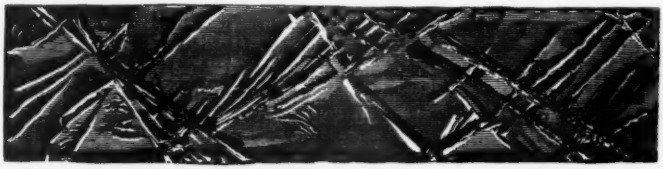
canic rocks, constituting, with a series of overlying metalliferous slates, the "Cobequid group" of the author, and resembling much more the Skiddaw and Borrowdale formations of the English geologists, than the contemporaneous Lower Silurian groups of inland America. There are, however, undoubted points of resemblance between these peculiar Silurian rocks of the Acadian Provinces and those of New England, and they constitute a remarkable instance of the difference that may obtain in contemporaneous deposits belonging to areas of quiet aqueous sedimentation and of igneous activity. They show very clearly how unsafe it may be, without proper caution, to apply the geological types of one area to those of another.

Below these peculiar Silurian rocks, are thick deposits of Cambrian age, on the whole less modified by contemporaneous igneous action, and in some places richly fossiliferous. In Cape Breton there have recently been recognized fossils indicating an Upper Cambrian horizon, resembling that of the English Lingula Flags. Below this is the Acadian series, so rich in *Conocoryphe*, *Paradoxides* and other forms of the Menevian type, étage C of Barrande. Still lower, according to the author, are the quartzites and slates constituting the gold-bearing series of the Atlantic coast of Nova Scotia. These have recently afforded some peculiar fossils, and appear to be a veritable equivalent of the European Longmynd and Eophyton sandstones. Some portions of this last group, associated with great masses and dykes of intrusive granite, have assumed the condition of gneisses and mica schists, with chiastolite and andalusite. Since the publication of the second edition of Acadian Geology, attempts have been made to relegate them to the Laurentian and Huronian ages, but good reasons are shown for the conclusion that they are merely metamorphosed members of the Lower Cambrian. There are, however, in New Brunswick, and, probably, also in Cape Breton and at the western extremity of Nova Scotia, true Huronian rocks, constituting an upper and lower series, and also true Laurentian, more especially in Southern New Brunswick and Northern Cape Breton. Details on these points, and references to the field geologists who have been working them out, will be found in the publication itself.

2. *Recherches expérimentales sur les cassures qui traversent l'écorce terrestre, particulièrement celles qui sont connues pour les noms de joints et de failles par* M. DAUBRÉE. (Comptes Rendus, lxxxvi, 1878).—M. Daubrée, whose experiments relative to meteorites were referred to in a recent number of this Journal (vol. xiv, p. 510) has completed another series of experiments, having as their object the explanation of the occurrence in rock-masses of joints and faults. The method employed was as follows: a plate of the substance to be examined, in the form of an elongated rectangle, was held at one end in a vice, and by the other extremity was subjected to a torsion or strain about a horizontal axis finally producing fracture. The extreme angle of torsion was 20°.

The best results were obtained when a plate of ice, 80 to 90 cm.

long, 35 to 190 mm. wide, and 7 mm. thick, was employed, it being enveloped in paper so as to preserve the position of the fragments. The most important results were: the formation of fissures whose traces on the outer surfaces of the plate were approximately parallel; there were often two *conjugate* series of these fissures, crossing one another at an angle varying from 90° to 70°, or less, and thus forming a net-like surface; in addition to the fissure-surfaces, there were also a small number of other planes of complete separation or fracture; and finally a series of fine straight lines, like the marks of an engraver, were observed on the surfaces. They were parallel to the fissures and often more regular than they; they indicated the existence of a distinct cleavage whose presence could be proved by a sudden shock. The cut represents a plate which has been subjected to the torsion.



The above results obtained by the torsion or twisting of a plate of ice are shown by M. Daubrée to be closely analogous to the phenomena of faults and joints observed in rock-masses. There is the same approximate parallelism among them; and this is true not only of faults upon a grand scale but also of secondary faults, and minor joints proving them all to have had a similar origin. Moreover it is very common to observe more than one series of these parallel fractures, analogous to the two conjugate series of artificial fissures mentioned above; and though a real difference of age often exists between them it must be in many cases true that they have been produced at the same time. The artificial planes of cleavage described have a parallel in the natural cleavage observed in many rocks, as for example that which gives rise to so-called grain in granite. In short, there appears to be a close similarity between the fractures of different kinds observed in rocks, and those produced artificially in these plates as the result of torsion. From this resemblance in effects M. Daubrée argues an analogy in the causes that have produced them. And he concludes that among the different kinds of mechanical actions, lateral crushings and so on, to which the crust of the earth has been submitted, torsion is one which has played a prominent part in connection with the production of faults and joints.

3. *Notice of a fourth new Phosphate from Fairfield Co., Connecticut;* by GEORGE J. BRUSH and EDWARD S. DANA.—In our further exploration of the locality mentioned as affording the three new phosphates described in the last number of this Journal, we have found a fourth new species belonging to the same group. The mineral is salmon-colored and proved on examination to be a phosphate analogous to triphylite in composition. It occurs

immediately associated with spodumene and albite and a mineral resembling Shepard's cymatolite. It has generally a bright salmon color and a sub-resinous luster. Hardness =4. Specific gravity =3·424. B.B. fuses at 1 to 1·5 coloring the flame bright lithia red, with streaks of green, and reacts with the fluxes for iron and manganese. Analysis by Horace L. Wells proves it to be a phosphate of manganese and lithia with about four per cent of iron, giving the formula $LiMnPO_4$ or $Li_3PO_4+Mn_3P_2O_8$. We propose to name this new mineral *Lithiolite*. A full description with analyses will be given in an early number of this Journal.

4. *Mineralogische Mittheilungen* (Neue Folge); von G. vom Rath. (From the Zeitschrift für Krystallographie, i, 6, 1877.)—Prof. vom Rath describes:—a remarkable compound crystal of bournonite, consisting of four individuals, but not a true twin; also some new forms upon calcite crystals of Bergen Hill, New Jersey; and crystals of a new mineral, Krennerite. This last mineral, a telluride of gold, was first described by Krenner under the name of "Bunsenin;" as this name has already been used for another species, vom Rath, who describes the crystalline form, proposes the name Krennerite after the discoverer. E. S. D.

5. *Das Erdbeben von Herzogenrath am 24 Juni, 1877: Eine seismologische Studie von* Dr. A. von Lasaulx. 77 pp. 8vo. Bonn, 1878, (Emil Strauss). The monograph of Prof. Lasaulx upon the earthquake of October 22, 1873, at Herzogenrath, near Aachen, was noticed in this Journal in Nov., 1874, at p. 392. The earthquake which took place on the 24th of June, 1877, in this locality, had in its phenomena much similarity with the former one, and it has been investigated in the same careful and systematic manner. As the results of the investigation it is concluded that the point from which the shock went forth was at a depth of 16·85 English miles, and that the velocity of propagation was 17·7 miles per minute, the general direction being southwest and northeast. The occurrence of the earthquake is regarded as more or less intimately connected with the great mountain-fissure—the "Feldbiss," which crosses the coal formation of the region of the Wurm in a direction nearly normal to its strike. E. S. D.

6. *Die Mineralogie* von Franz von Kobell. 5th edition, 252 pp. 8vo. Leipzig, 1878. (Friedrich Brandstetter.)—The Mineralogy of von Kobell is now too well known to need commendation here. In the present edition the article upon the chemical constitution of minerals has been altered to some extent with reference to the now accepted chemical principles.

III. Botany and Zoology.

1. *Early Introduction and Spread of the Barberry in Eastern New England.*—On the 10th of June, 1764, the Province of Massachusetts passed "An Act to prevent damage to English grain, arising from Barberry-bushes," with the preamble: "Whereas it has been found, by experience, that the blasting of wheat and other English grain, is often occasioned by barberry-bushes, to

the great loss and damage of the inhabitants of this province; be it therefore enacted," etc.

It is remarkable that this shrub, which is naturalized only along the sea-board of New England, mainly in Massachusetts, and which has up to this time failed to spread anywhere in the interior, should have so multiplied in Massachusetts during the first hundred years as to have made this trouble or obtained this evil reputation. An apparent justification of this ill odor of the Barberry (among the agriculturists we mean) will be found in this Journal, vol. xlix of the second series, 1870, p. 406. Mr. Goodell of Salem, the editor of these old Province Laws, who called our attention to this Act, writes:

"The barberry had, evidently, been widely and abundantly propagated in the older settlements, during the century or century and a quarter of their existence; and it is reasonable to suppose that attempts to exterminate the bushes were not limited to the period of this act's continuance. When the cultivation of wheat, particularly, declined, the farmers, probably, relaxed their efforts against the barberry, and hence may we not account for its present comparative abundance in the vicinity of the older towns, which were near the seaboard? I remember, when a boy, of seeing a single barberry-bush in Athol, Mass., transplanted from somewhere in the eastern counties. It was there considered a remarkable curiosity; but I remember that I wondered why the barberry should not have traveled as far west long before, seeing that the pioneers of that region were, largely, from Essex and eastern Middlesex, and that the bush appeared as thrifty as any I had ever seen in Essex County. The preamble of this act, perhaps, explains the mystery; for there is no reason to doubt that the 'experience' of the farmers therein mentioned was neither recent nor confined to a few, in 1754, when the Legislature, representing a large number of country towns, deemed it necessary to take such vigorous measures for extirpating the barberry."

It has been suggested that the barberry never really damaged the grain-crops of New England, at least to any notable extent; but that the settlers, bringing with them from England the popular fear of it, legislated upon that. But if so, we have a curious illustration of the precarious nature of testimony. For in Europe, this colonial legislation has passed into history as independent evidence that the barberry did damage grain in New England.

A. G.

2. *Ferns of North America;* by Prof. D. C. Eaton. Parts IV and V, issued together, bring up the letter-press to p. 113, and the illustrations to plate 15. All but two of these plates carry a couple of species, and the letter-press grows more copious. *Aspidium Nevadense* well fills a plate, and is capitally managed; it is a new species of the Sierra Nevada, the joint discovery of Mrs. Austin and Mrs. Pulsifer Ames, whose names are thoroughly identified with California botany. *Pellæa densa*, of Oregon and California (which does well in cultivation), and *P. pulchella*, a

species which extends from Peru to Western Texas, make a good plate. The supposed raphides of the upper surface of the former may be cystoliths. *Cheilanthas viscida* of Davenport, a new species of Mr. Lemmon's discovery, and *C. Clevelandii* of Eaton, discovered by D. Cleveland of San Diego, form the best plate of this issue, except perhaps that of *Aspidium unitum*, var. *glabrum*, which turns up in Florida. *Anemia Mexicana* and *A. adiantifolia* are well given. *Asplenium Ruta-muraria* is fairly well done, and *A. septentrionale* makes little show at best; and what serves for analysis of both is wretched. A. G.

4. *Dictionnaire de Botanique;* par M. H. Baillon. Paris. (Hachett & Co.).—This work has proceeded to the eighth fascicle, to p. 640, and to near the end of Ca. The affluence of illustration continues. A. G.

5. Vargas *considerado como Botanico*, etc., por A. Ernst. Carãcas, Dec., 1877. 4to.—This is a memorial discourse upon Dr. Vargas, pronounced before the Venezuela Society of the Physical and Natural Sciences, upon the occasion of the translation of his remains to the National Pantheon. To this is added the correspondence of Vargas with De Candolle and his friend Mercier; a list of the plants mentioned in the Prodromus as coming from Vargas; and, finally, the Candollean genus dedicated to Vargas having been suppressed by the present writer, a new genus, *Vargasia*, of *Ternstræmiaceæ*, near *Marcgraria* and *Ruyschia*, is proposed and characterized by Dr. Ernst, and two species described. A. G.

Dr. Thomas Thomson, the school-mate and associate, in travel and publication on Indian Botany, of Sir Joseph Hooker, son of the distinguished chemist and professor at Glasgow half a century ago, died at London, April 18th, after a long illness. The Gardener's Chronicle of April 27, gives an appreciative biographical notice. A. G.

IV. Astronomy.

1. *Transit of Mercury.*—The transit of Mercury was observed at New Haven on the 6th of May by Messrs. J. J. Skinner, W. F. Beebe and H. A. Hazen. The following are the results of the observations in Washington mean time; the phases being those described in the Washington instructions. Clouds prevented observations at some of the contacts.

	External Contact.	Phase I.	Phase II.	Phase III.	Diam. glass.	Power.	Obs.	Location.
	h m s	h m s	h m s	h m s				
Ingress	22 4 43·	22 7 17·	22 7 32·	22 7 42·	4·6 in.	90	S.	N. Sh. Hall.
Egress	5 36 54·	5 33 58·	5 33 48·	5 33 18·				
Ingress	----------	22 7 19·2	22 7 32·1	22 7 49·4	5· in.	180	B.	Athen. Obs.
Egress	5 37 2·2	----------	----------	5 32 44·2				
Ingress	----------	----------	----------	----------	9·6 in.	210	H.	S. S. S. Obs.
Egress	5 36 51·1	5 33 57·8	5 33 48·8	5 33 30·8				

V. Miscellaneous Scientific Intelligence.

1. *The International Geological Congress.*—The time for the opening of this Congress in Paris is now finally fixed, by the local committee, for the 29th of August, and the Congress will remain in session about a fortnight. Further details as to organization and place of meeting will soon be made public. Meanwhile, it is announced that from the 20th of August to the 15th of September, the library and reading-rooms of the Geological Society of France, No. 7 rue des Grands-Augustins, Paris, will be at the service of members of the Congress. As before, it is requested that all those who desire to take part therein will make it known to the General Secretary, Dr. Ed. Jannetaz, at the above address, where, also, the subscription of twelve francs, required for each member, may be sent to Dr. Bioche, treasurer. Ladies are admitted to the Congress.

The local committee add to the above announcement as follows: There is reason to believe that the numerous collections of geology and paleontology (minerals, rocks, fossils, maps, plans, sections, models in relief, etc.) to be found in the *Exposition Universelle*, will realize the expectations, expressed in the circular of the International Committee, of an International Geological Exhibition. All exhibitors of such collections are requested to send, as above, such lists as will enable the secretary-general, Dr. Jannetaz, to prepare a special catalogue of them for the use of the Congress.

T. Sterry Hunt, *Secretary of the International Committee.*

2. *Session of the National Academy of Sciences in April.*—List of papers read before the National Academy of Sciences, at the April session, 1878:

Formation and structure of Alacrane Reef on the Yucatan Bank; by A. Agassiz.

The theory of waterspouts; by William Ferrel.

Report on the orbits of the satellites of Mars; by Asaph Hall.

On the relation of loess and drift to secular disintegration; by R. Pumpelly.

On the characteristic invertebrate forms of the central zoo-geographical province of the United States; by A. S. Packard, Jr.

On an optical ocean-salinometer; by J. E. Hilgard.

Preliminary report on the deep sea dredgings of the U. S. Coast Survey steamer "Blake" during the past winter in the Gulf Stream and the Gulf of Mexico; by Alexander Agassiz.

Abrasions on the northwest coast of America; by George Davidson.

On the law of Boyle and Marriotte; by Wolcott Gibbs.

Abstract of a memoir on the intersection of circles and the intersection of spheres; by Benjamin Alvord.

Biographical memoir of Louis Agassiz; second part. Relating to his life and work in America; by Arnold Guyot.

Biographical memoir of Jeffries Wyman; by A. S. Packard, Jr.

Plan for measuring the velocity of light; by Simon Newcomb.

On the force of effective molecular action; by William A. Norton.

Remarks on the value of the result obtained for the solar parallax from the English telescopic observations; by C. H. F. Peters.

On the vertebrate fauna of the Permian period of the United States; by E. D. Cope.

Report of progress on the subject of "oxygen in the sun;" by Henry Draper.

Photometric comparisons of the components of close double stars; by E. C. Pickering.

On the duplication of geographical names; by F. V. Hayden.

Characteristics of some of the lower spectral lines; by S. P. LANGLEY.

A new element of the cerium group; by J. LAWRENCE SMITH.

On the primary zoo-geographical divisions of the globe and their relations; by THEODORE GILL.

Mr. Wallace and Mr. Allen on geographical distribution, with special reference to the alleged distinctness of the Nearctic region; by ELLIOTT COUES.

On the structure and origin of mountains, with special reference to recent objections to the contractional theory; by JOSEPH LECONTE.

Photometric measures of certain faint stars and satellites; by E. C. PICKERING.

Contributions to meteorology (ninth paper); by ELIAS LOOMIS.

Recent displacements in Utah; by G. K. GILBERT.

On the laws governing the movements of the Rocky Mountain locusts; by C. V. RILEY.

Supplementary notice on the paper, "Whence came the inner satellite of Mars," read at the October session, 1877; by STEPHEN ALEXANDER.

3. *Bulletin of the United States National Museum.* Department of the Interior. No. 10. Contributions to North American Ichthyology, by D. S. GORDAN. No. 2. 120 pp. 8vo, with 45 plates.

4. *Payen's Manual of Industrial Chemistry*, edited by B. H. PAUL. 987 pp. 8vo, with 698 figures in wood. New York, 1878. (John Wiley & Son).—This translation of Payen's well-known "Précis de Chemie Industrielle," is an acceptable addition to our chemical literature. We learn from the title page that it is "based upon a translation (partly by T. D. Barry) of Stohman and Eugler's German edition" of Payen. The work of the translator is well done and the whole work, as a systematic treatise upon industrial chemistry, has a high value. To those who possess few books in chemistry the addition by the editor of several chapters on general chemistry will be found valuable. The volume extends to 987 pages octavo, and the text is in fine type—too fine to be convenient for many readers—so that the amount of matter contained in the work is very large. There are omissions, as, for example, the facts relating to metal plating; those connected with the zinc process for silver production; and some of the recent improvements in the sulphuric acid processes. Being a French work, the neglect to mention American methods in technology is not a surprise. The work is still indispensable to all interested in the industrial chemistry.

5. *Smithsonian Institution.* — The office of Secretary of the Smithsonian Institution, left vacant by the decease of Professor Henry, has been filled by the appointment of Professor S. F. Baird, who has for a long time held the position of Assistant Secretary, and is eminently fitted for the higher place by his long and active experience in the affairs of the Institution.

6. *American Association.*—The American Association for the Advancement of Science will hold its next meeting in St. Louis, commencing with the third Wednesday in August. Professor Marsh is the President of the Association for the meeting and the ensuing year.

7. *British Association.*—The next meeting of the British Association will be held in Dublin. It will open on the 14th of August.

8. *Rate of Earthquake Wave Transit.*—Under this title, in the Philosophical Magazine for May, Mr. Mallet has a reply to General H. L. Abbot's paper on page 178 of this volume.

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